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MOSQUITOES AND MALARIA.

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The only excuse the writer can offer for this brief paper is that now since malaria has become a household word, and has also become, as it were, a safe refuge for many of us when in doubt, it will not be amiss to note the progress which has been made in the etiology of this disease, and in particular to the role which the mosquito plays in the transmission from person to person.

It is now a little over two decades ago since Laveran described the parasite, and since then our knowledge regarding its behavior and life history was added to little by little. However, as valuable as these discoveries have been, little or no progress was made in determining just how, and in what manner this parasite gained entrance into the body, and caused the train of symptoms we ascribe to malaria.

Of the ten known species of the hemamoeba affecting the vertebrates, only three* are classed as affecting man. Laveran believed the malarial parasite to be one species, while Golgi asserts there are at least two, if not three or more; Celli recognizes three, the benign quartan and tertian, malignant tertian or æstivo-autumnal. Mason describes five: quartan, tertian, benign forms which do not form crescents, the quotidian pigmented, quotidian unpigmented, the tertian, all malignant form crescents. The plurality of species of the organism is further confirmed by Mannaberg's experiments, in which he inoculated thirty-three persons with blood from cases suffering from malarial fever, and produced in every instance the same type of the organism as were found in the blood of the original cases. The inoculations made by Bignami and Grassi with blood of a patient containing the tertian parasite also confirm this view. The experiment conducted by Gambon and Lowe is perhaps of greater importance than any of the above-sited. These gentlemen while at Ostia fed mosquitoes on the blood of a case infected with the benign tertian parasite, and sent these to England, where Dr. Thorbourne Manson submitted himself to be bitten by these mosquitoes, and in five days afterwards came down with an attack of the tertian fever.

In regard to this plurality of species, the writer has believed for many years that the contention of Laveran and others was incorrect, and based this belief on an observation of several hundred cases of malarial infections treated at the Marine Hospital at New York in 1889 to 1890. In the study of the blood of cases the morphology and development of the parasite was such as to suggest that there were two, if not three species,—the tertian, æstivo-autumnal and another unclassified now recognized as the quartan. While our views were convincing to ourselves, and not to others, they were not given more publicity than the occasion required.

Various theories from time to time

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*Since the above was written, a hæmatozoan has been reported as the cause of spotted or mountain fever, and a mycoid body found in certain cases of remittent fever.

have been advanced to explain how malaria was contracted. Among the oldest is that the mosquito is one of the agencies of transmitting it to man. In olden times it was held by several authors that malaria could be transmitted by the stings of insects. Varro described the mode of infection by inhaling or swallowing a small animal which grew in the marshes. But it was usually regarded as a miasm, as the name indicates. There were also popular beliefs regarding the mosquito being causative of malaria. The natives of East Africa have long asserted that it was caused by its sting, in fact, their name for malaria was "Mbu" the same as for mosquito (Koch). The natives of certain districts of India, where malarial diseases prevail, always sleep with the whole of their bodies and heads covered to ward off malaria. "The peasants of the Argo Romano always fumigated their cabins before occupying them, for the purpose of driving out the mosquitoes, believing that if they did not they would contract malaria."—(Celli.) In this country the belief is held "that to go out after sundown one is almost sure to catch the chills." Nott in 1848 speaks as if the mosquito theory was an accepted fact. To King (1883) belongs greater credit than to any other for the mosquito theory, and his arguments therefor. Then follows other theorists, Laveran and Flugge, 1891; Koch, 1892; Manson, 1894; Mendini, 1896, who drew an analogy between the mosquito in its relation to filaria sanguinis hominis, and the possibility of malaria being contracted in the same way. The most important of all are the facts adduced by Ross, 1898, by which he demonstrated that bird malaria was infected through and by the mosquito. Ross' researches on the proteosoma infection of birds were such as to suggest the mosquito being the agent in transmitting human malaria. In order for a better understanding of the importance of Ross' discoveries, it may be well to refer to these a little more in detail. In 1898 he took 245 gray mosquitoes (genus culex), which were allowed to feed on birds which were infected with proteosoma. One hundred and seventy-eight, or 72 per cent, consequently contained certain pigmented cells in their stomach walls. Two hundred and forty-nine other mosquitoes, used as a control to the above, were fed on men infected with æstivo-autumnal parasites (crescents) on immature tertian, on birds with halteridum infection and on healthy sparrows. All were negative. But of eighty-one mosquitoes fed on mature proteosoma seventy-six showed pigmented cells. His further observations were that these pigmented cells after being lodged in the muscular walls of the mosquito's stomach became nucleated, began to enlarge to three or four times its size, and after a lapse of from six to eight days these nucleated bodies became further subdivided into "germinal rods," when the corpuscles burst outwards, setting free the rods which were taken up by the body fluids, and finally were found in the salivary gland. Birds bitten by mosquitoes after a lapse of from six to eight days (the time required for the parasite to complete its sexual development) would almost invariably be infected. Another interesting feature was by feeding a mosquito on several occasions, there would then be several crops of the organism developing in their various stages.

Many of the Ross experiments with regard to malaria, however, were negative, but he finally found a few mosquitoes belonging to a particular species (anopheles), in which, after having been fed on malarial blood, there was observed after four or five days pigmented cells in the stomach wall, which he had not observed in non-infected insects. After finding the proper mosquito, it was then any easy matter to observe the development of the parasite through its various stages. Grassi and Bignami, who had been working on the same lines as Ross, determined the species of mosquito prevalent in districts where malaria prevailed, and succeeded after a second attempt in infecting persons through them. MacCallum and Opie, in 1897 and 1898, made researches on the proteosoma and Halterdium infection of birds, the latter the æstivo-autumnal infection in man, and discovered that the parasites possessed two forms,—one a pigmented cell which became flagellated soon after its removal from the body within fifteen to twenty minutes, and another in which the pigment was distributed in a definite ring not diffused. The flagellum concentrated its pigment at one end, dashed in to one of the large granular pigmented spheres which seemed to throw out a pseudopodum to meet it, buried its head, finally wriggling its whole body into it. Soon thereafter the body became round and crescent.. Here he was able to observe practically what occurs in sexual forms of the malarial parasites when taken into the stomach of the mosquito. His interpretation of these phenomena was that the two forms found in the blood of birds were the male and female elements, and this process was that of sexual conjugation. Following these observations he examined human blood containing the æstivo-autumnal parasite, and was able in two instances to observe a conjugation of these bodies, which occurred within a few minutes after the blood was taken.

The question would then naturally suggest itself, whether conjugation of these male and female elements can or does take place under same circumstances in the human and bird species, and is not altogether dependent upon an intermediate host. The malarial parasite, like the coccidia, requires a host to complete its cycle of development, being parasitic to man and to a certain genus of mosquito (anopheles). It has two cycles, the asexual cycle of Golgi —endogenous (Golgi), and an asexual cycle, exogenous (Ross). The endogenous mode of reproduction is one of spore formation and can be carried on for a period more or less long without the agency of the mosquito. The other, the exogenous, is a sexual process which can only be completed by the passage through a definite host, the mosquito. The endogenous cycle of the parasites is completed in from twenty-four to seventy-two hours, according to the class to which it belongs.

In general, if blood of a patient be examined shortly after, some of the red cells will contain small activity, motile amœbula, and at the end of twelve hours these have increased considerably in size, often occupying as much as a third of the corpuscle. It now contains a few granules of pigment. At the end of twenty-four hours the ameboid movements are less active, gradually becoming less active, and the pigment becoming more concentrated, the body now segments (sporocytes) and destroys the blood cell and is set free in the plasma. The time required for the sporulation is: for the quotidian 24 hours, tertian 48 hours, æstivo-autumnal 48 hours, quartan 72 hours. The number of sporocytes formed in each segmenting form are tertian 15 to 23, æstivo-autumnal 5 to 12, quartan 6 to 14, in addition to the sporocytes the æstivo-autumnal from the well-known spheres and crescents after six to eight days, the large pigmented spheres in benign tertian in from one to two days. These two latter are the forms of parasites which seem necessary to complete the exogenous sexual cycle.

According to Sambon, the formation of the asexual spores during the swarming stage, is analagous to the production of zoospores in various protophytae. The segmentation of the parasites is the result of the exhaustion of nutrition, the process being in all manner and purpose the same as parthogenesis in certain of the aphides. These insects may produce this by parthogenesis throughout the summer season, but at the beginning of the cold season male and female develop, conjugate for the perpetuation of their existence through an egg which will withstand the rigors of the winter. This naturally raises the question whether conjugation of the male and female elements of the malarial parasite takes place within the body, and whether this does not occur under certain circumstances, as it is a well-recognized fact that many cases of malarial infection terminate in recovery or a stage of latency, even when no anti-periodics are administered. The apparently recovered person may be subject to another attack at weeks' or even months' duration afterwards, when the blood immediately after withdrawal, if examined, will contain both the male and female elements, that is to say the flagellated and the large pigmented bodies. Under these circumstances, is it not an improbability that the parasite in its existence within the body may complete its asexual cycle?

Many anopheles evidently do not transmit malaria or may be capable of doing so under some circumstances and not under others. Ross attributes his failures probably to climatic conditions, although it must be said that the anopheles Rossii, which is the most numerous in some places, have not been able to be infected with the malarial parasite. Buchanan's observations on quartan ague by the bite of infected anopheles are as follows: From the observations now on record it would be inferred that it does not always follow that mosquitoes are easily infected, and that in many instances fails. This may be partly due to the mosquito in being in proper condition to receive the parasite, or more probable the parasite is not in the proper stage of development for conjugation. Grassi believes that the sexual elements may undergo a parthogenesis and may not be in condition to furnish the male and female elements. Further, he says there are persons whose blood does not allow the fertilizing forms to develop, and therefore, are incapable of infecting the anopheles. Liston (Indian Medical Gazette, April, 1902) summarizes the matter thus:—

1. The man experimented on might be immune.

2. The anopheles did not bite at the time when the parasite was in a suitable condition.

3. The season was not one in which the quartan parasite could develop properly.

4. The species of anopheles used in the experiment was not the correct one.

When a young mosquito escapes from the egg, which floats on the surface of the water, it is a worm-like larva. It swims, eats greedily, grows rapidly, shedding its skin several times until it has attained its full development of the pupa state, during which it ingests no food, but undergoes certain anatomical changes, just as we see in the chrysalis stage of other insects. The culex mosquito differs from the anopheles in the manner in which it deposits its eggs. These consist of from 200 to 250 eggs glued together in a boat-shape-like mass, whereas the anopheles' eggs are deposited separately and are not adherent to one another. When examined with a lens the culex egg is seen to be rather long and narrow, more pointed at one end than at the other, and is attached to other eggs in its long axis, that is to say, the broader end of the egg lies next to the surface of the water. The anopheles' egg appears as an oval body, hardly large enough to be detected by the naked eye. It is, like the culex egg, broader at one end than at the other. The egg, however, is not completely circular in its transverse section, one side being more rounded than the other. This allows the egg to float with the rounded side on the surface of the water, the more flattened or slightly concave surface being uppermost. The appearance somewhat resembles that of a boat, which occupies a little more than one-third of each side of the egg. The air cells act as a float to the egg. The larva escapes from the egg under favorable circumstances, within two or three days. Bruno and Galli-Valerio say that the larvæ of both the anopheles and culex may pass the winter in pools, even when covered with ice, but this is of rare occurrence. These larvæ are usually those of anopheles punctopennis and those of maculatus do not survive. Dry eggs of both genus are not affected by freezing (Grassi).

With regard to a further discussion of the life history of the mosquito and the means of determination of genus and the several species, it would not only be long but encroaching too much on the prerogative of the entomologist, who is far more able to do the subject justice than the writer. Only this general statement will be made relative to the breeding. The anopheles seems to prefer pools to fresh water, running streams, water in growing plants, whereas the culex will deposit her eggs on anything that contains sufficient moisture to hatch them. The period of incubation in the several forms of fever according to Caccini, states for the tertian is from five to eighteen days, æstivo-autumnal five to nine days, malignant tertian five to nine days, quartan has not been determined.

Latency.—Malarial infections may appear to have a period of latency for a few days, which may extend over many months. This latency may be to all intents and purposes the same as a symposis and may resemble the symposis of the parasite of Texas cattle fever. The exciting causes of malarial fever attacks may be fatigue, traumatism, sudden chill, foods causing gastro intestinal irritations, medication, change of climate, inter current sickness, typhoid, dysentery, pneumonia, etc.

Immunity.—With regard to immunity to malarial infection, it is a question whether this actually exists in the same sense as we ascribe the state or resistence to other infectious diseases. There may be a limited number who are non-infectible. There may be, on the other hand, many who harbor the parasite without giving any clinical expression of its presence. It was an almost accepted fact that people living in a region where malaria is endemic became immune. This applied particularly to the natives of Africa living on its western coast. Here the adult population appeared not to suffer from malaria, many of the children, however, suffer with malarial infection in all its forms. The majority of these, however, are not attacked. Koch's observations on both adults and children showed that the majority were infected with the same species of parasite as was fatal to the Caucasian, yet with apparently no ill results. There must, therefore, be in a relative sense what is a clinical immunity but not actual. In fact, the same state appears to have been established in these races as in bovine malaria—a state of symbiosis, wherein the parasite can exist within the body for an indefinite time without disturbing its equilibrium. Not long ago many prominent clinicians were inclined to believe that the so-called mixed infections were a misnomer, and that the condition termed typho-malarial fever was purely typhoid fever. In all probability the majority of these cases so termed are typhoid fever, but there are cases encountered which are mixed infections—malaria and typhoid. The clinical symptoms sometimes resemble malaria and at other times those of typhoid.

If the writer may be pardoned for a slight digression from the title of this paper, he would refer to some cases of this character which were, it is believed, the first to be recorded.

"Squeir, in an article published in the American Journal of Medical Science, 1885, discussed at length this peculiar form of fever, and his conclusions were that it was a distinct type of fever, and not one of a mixed infection of typhoid and malaria. In the early part of April, 1890, I reported in the abstract of Sanitary Reports a preliminary note on what I termed entero-malarial fever. Six cases of mixed infection were encountered in a series of examinations; over 100 of malarial cases, ten occurring during the latter part of 1899.

Connected with this series of malarial cases, several others of mixed infection have been encountered, a combination of malarial and enteric fevers, presenting clinically some deviations from the general course of either disease, and deemed of sufficient importance to record.

No difficulty has been encountered in establishing the source and time of malarial infection, but with regard to the enteric infection the difficulty has been great. Generally, however, the history was to the effect that the patient had remained ashore for some time previous to sailing to the malarial districts. This combination of these diseases has presented two sets of symptoms dividing the cases in two groups: 1. Cases in which the symptoms of malarial fever predominate, masking the enteric symptoms. 2. Cases in which the symptoms of enteric fever are more prominent.

In the first group (two cases), at the onset of the attack the patients presented clinically all the symptoms of malarial fever, remittent, giving a clear history of infection, the attack being characterized by a chill followed by fever and remissions, constipation and irritable stomach, etc.; this chain of symptoms completely masking the graver trouble, so that suspicions are not aroused as to the true character of the malady until the patients were under observation for four or five days, or even longer. On admission, the blood of the patients was examined for the malarial organism, which being found in abundance, the cases were put in appropriate treatment. Æstivo-autumnal and tertian parasites.

A brief synopsis of the symptoms of one case will serve for both: H. M. aged 23. Was taken sick two days before admission, the attack commencing with a chill followed by fever, marked by a remission. On the day of his admission he had chilly sensations, then a rise in temperature followed later by remission. A careful physical examination revealed nothing abnormal except a slight tenderness of the epigastric region, and a considerable enlargement of the spleen. The bowels were constipated. A microscopical examination of the blood was made, and a large number of the plasmodia malariæ, æstivo-autumnal were found free in both the serum and within the blood corpuscle. This established the diagnosis of malarial fever of the remittent type. On the fifth day after admission there appeared on his abdomen several suspicious-looking spots suggestive of enteric complication. On the day following he had slight epistaxis and a tendency to diarrhea, tenderness in the right iliac fossa. At this juncture, a bacteriological examination was made of the feces, and after several trials a bacillus answering to the description of that described by Eberth was isolated from the stools. An examination of the blood at the time demonstrated the presence of the malarial parasite, but in greatly diminished numbers. During the next week the enteric symptoms became so marked that without any microscopic examination there could be no doubt concerning the enteric fever. At this time the remissions had ceased, and the temperature curve was characteristic.

In the second case (group 1), the symptoms were not so pronounced as in the first, but it did not differ from it on the whole. The plasmodium malariæ was found in the blood in abundance, and later the typhoid bacillus was isolated from the stools.

In the second group of cases (three in number) the enteric symptoms were well marked, giving a clear history of the disease. The patients had just returned from the south, where malaria was prevalent. The history gave the prodromal period, lassitude, etc., followed by diarrhea, epistaxis, and tympanites, and in one case slight hemorrhage. As a matter of routine the blood was examined for the parasite, which was found confined in the corpuscle, not free in the blood serum, tertian. A bacteriological examination was made in each case, and the bacillus typhosus isolated from the spleen, thus establishing the co-existence of both factors in the disease.

In one of the cases, during the third week of the attack, when convalescence appeared to have been established, the temperature being normal and the appetite returned, he had a sudden elevation of temperature that rose to 39° and lasted about four hours. The cause of this was attributed to some indiscretion, a not infrequent complication during convalescence from enteric fever.

In twenty-four hours after the first attack he had another similar in all respects, which suggested that the probable cause was malarial. The blood was again examined and the plasmodium found to be present. A return to antiperiodic treatment for a few days, and then the case went on to speedy convalescence. The other cases of this group terminated fatally, one by peritonitis following perforation, and the other by pneumonia. The combination of the two causes appears to have produced a more adynamic form than has been observed in other cases with like symptoms occurring here during the past two years."

Prophylaxis.—There can be no reasonable doubt that the mosquito is the sole agent in distributing the malarial parasite, as well as the only means by which it completes its oxogenous cycle.

The distribution of malaria goes hand in hand with that of the genus anophelidat, and without it there can be no malaria (Grassi). The existence of the mosquito may be denied by the natives, but there is no instance on record where it has not been found when looked for by competent persons.

As before referred to, the mosquito as intermediate host is harmless unless it has access to the parasite. Koch states that infected human beings be considered the starting point of new cases. Only a small number of mosquitoes can survive a winter. These must be re-infected in the spring. There have been a few advocates of the theory that malaria prevails endemically in some portions of the tropics, and is independent of the human species. Christopher's and Stephen's observations go to show that this is erroneous, particularly so with regard to the explorers in the jungles of Africa. They say there are but few places where these are not always near the natives.

All mosquitoes, even after feeding on infected blood, are not able to transmit the parasite to man. Some anopheles are more easily infected than others. Some not at all. Frequently the parasite does not complete its sexual cycle in the mosquito because the gametes are either too young or too old, or even when all conditions are satisfied its development may be arrested by meteorological conditions. Anopheles, however, does not necessarily mean malaria any more so than the presence of the stegomeia means yellow fever. Nutall has already pointed out this fact. Many districts in England are infected with anopheles, yet malaria does not prevail. The same has been observed in Italy by Celli, where certain localities are infected with malaria, while in others contiguous thereto, anopheles being in abundance, are exempt. The same has been observed in a locality in northern Michigan where anopheles have been found, having been exempt from malarial infection for years, became infected to a slight extent by a latent case among a gang of railroad laborers.

The distribution of the several forms of malaria, while dependent primarily upon the mosquito, is influenced very largely by climatic conditions which may be in themselves sufficient to prevent the development of the parasite. The pernicious forms of malarial infection are not indigenous to the northern part of the United States, even in localities where the benign forms prevail. Many cases of the pernicious tropical malaria were imported into the United States, during the late Spanish-American war, and from the Philippine Islands, and were distributed to nearly every state and territory, yet notwithstanding there were no foci originating from any of these, it is then quite evident that climate must in some way have modified the parasite, either in its endogenous or exogenous cycle that it could not be perpetuated. It is more than probable that the modifications occurring in its endogenous cycle cause secondary cases of the benign tertian as occur in these places. Shoo suggests as an explanation why certain districts are immune to malaria, while others contiguous thereto may be infected, may rest on the natural food of the mosquito. His observations on malarial infection in Northern Holland in 1902 were to the effect that he was unable to infect anopheles when these were fed on acid fruits, such as ripe tomatoes. It mattered not whether the insect was fed before or after infection, but if they were fed upon food such as water melons or sugar kept in plain water, they could with approximate certainty be infected. Celli makes a similar observation regarding the distribution of malaria in certain districts of Italy. Some districts are given up very largely to the cultivation of tomatoes. The persons living in such are free from malaria, whereas those living contiguous thereto, not cultivating these, are infected, the same species of anopheles being prevalent in both places. It has also been suggested that possibly the cultivation of the potato, the plant being quite acid, has some bearing upon the prevention of malaria in England. Before its cultivation became general, malaria prevailed in England quite extensively, and now it is extinct.

Many persons appear to be immune to malaria, and to all practical purposes they are so, if one is to judge simply by the clinical manifestations. It was believed at one time that the natives of Africa were practically immune to malaria, and the negro was nearly so to yellow fever. Koch's researches on malaria on the coast of Africa show that this immunity is a relative one. He found the majority of the negro children infected, and while the adults were not so much so, the children often had fever. Many were to all intents and purposes in good health. These parasites were of the same character as seen in the pernicious forms of fever occurring in the Caucasian.

These cases of apparent immunity are those which undoubtedly are the cause of perpetuating malarial infection in a given place, or for its distribution over wide and separate areas. The great influx of foreign population, much of which is drawn from Southern Europe, where malaria is endemic, furnishes a large number of latent infection, and has no doubt been the means of establishing new foci as well as increasing the infection in places where it had formerly prevailed to a slight extent.

The management of these latent cases is perhaps one of the most difficult problems with which we have to deal in eradicating malaria. In the first place, the majority of such cases never come under the control of the physician, only when there is a sharp clinical expression of the disease. Many know its symptoms so well that after taking a few doses of quinin they resume their avocations and resort again to self treatment on another attack. Many others so affected pursue the same course after consulting a physician and do not persist in a prescribed course of treatment sufficiently long to remove the parasites. On the other hand, many physicians do not persist on a course of treatment which would accomplish this, but are content to meet these emergencies as they arise, and would appear as indifferent as to the final termination of the case.

The efficacy of wholesale administration of the cinchona salts has been frequently proven for keeping the infection at a minimum. The daily administration of even a small quantity is sufficient for this purpose, but it does not always suffice to remove the parasites from the blood nor prevent a reinfection. It has been a time-honored custom of physicians living in the Southern States to administer quinin over a considerable period, and at intervals of about a week, commencing a few days after the cessation of the fever, that is to say a full dose on the 7th, 14th, 21st, and 28th days. This method has recently been advocated by none less than Koch. If such a plan could be adopted and made universal it would not be long until the mosquitoes would be freed from infection. Such a scheme, however, would fail in its object if we did not provide for the control of infected persons arriving from other places, or preventing the importation of infected places.

It would be next to impossible to maintain this status, because of the ever increasing and rapid intercourse between all sections of this country, without great difficulty and hardship. While popular sentiment would approve of measures to prevent the introduction of yellow fever from foreign countries or in times of an epidemic here from one place to another, it would not acquiesce to an antimalarial measure, unless under some such guise. No radical measures, therefore, could be enforced at the present time unless there was a complete change of popular sentiment regarding the gravity of malarial fevers. Quite a number of plans have been suggested for the eradication of malaria, the drainage of swamps, the cultivation and drainage of soil, changing the nature of soil, planting certain trees and shrubs to abstract the mosquito therefrom, and personal prophylaxis (Celli).

Koch advocates personal prophylaxis as being the most important, while Ross believes in an anti-mosquito campaign and personal prophylaxis, i. e., preventing the mosquitoes infecting persons and the isolation of the cases for a period as long as they are capable of transmitting the parasite to the mosquito. All these plans have to a more or less extent been put into execution, and have been of benefit. So far, however, none have entirely banished malaria. Draining swamps is the oldest plan on record. Empidocles in 460 B. C. drained the marshes around Agegentum for this very purpose.

Quinin enjoys the distinction of being the universal remedy for the prevention as well as the cure for malaria. Other remedies have been advocated from time to time, but none has so far approached it in efficacy. Probably there may be discovered a better remedy now since the etiology of malaria is better understood. The life history of the mosquito is attracting the attention of the scientific world.

The question as to whether susceptible persons can remain for a considerable time in a malarial locality without contracting the disease needs no further discussion other than to refer to certain experiments of the commission sent out by the School of Tropical Medicine of Liverpool and those of the Japanese Government. The commission repaired to Ostia, a place notorious for its fevers, and remained there for over three months, during the time of the year when malaria fevers were most prevalent. Its members observed no other precaution than guarding themselves against mosquito bites. All escaped infection. During this time others coming into this place, not being protected, nearly all contracted malaria. The other instance is the experiment of the Japanese Government with a regiment sent to Formosa in 1901, at the season when the fever prevailed. Two battalions were selected, one was protected against mosquitoes, and the other allowed to live under the ordinary condition of camp life. Observations were made on these two battalions for over five months. The protected batallion escaped from all forms of malarial infection, while the unprotected had 259 cases of fever.

The wonderful success achieved by the United States Army during its occupation of Havana has no paralell in sanitation. Its first effort was to clean up the city and make it as clean as possible. It was only a short time until this was done, for it was a universal belief that the reason why yellow fever constantly prevailed in Havana was due to its filth. Despite the most painstaking and thorough cleansing of the city, the military and sanitary authorities were grievously disappointed on the net results of this work. Yellow fever showed no diminution, but on the contrary had prevailed to a larger extent than before the occupation. Malarial fevers also were as frequent. Just at this juncture a commission, headed by the late lamented Walter Reed, began its researches on the etiology of yellow fever. Soon after his wonderful discovery became known that the stegomeia mosquito was the intermediate host of the cause of yellow fever, his recommendations were adopted and put into effect. The campaign was simply directed against the mosquito, sanitation being secondary. It was practically a war against the mosquito, both to prevent its breeding and preventing its biting cases of yellow fever. Within less than one year after this campaign had been instituted, yellow fever was eradicated from Havana. The result was that not only yellow fever was eradicated, but the deaths from malarial fevers dropped down to almost nothing in comparison to what had existed in former years. Havana to-day enjoys the distinction of having a death rate less than any other city in the tropics. The military rule of Cuba was, therefore, not only a political godsend to the people of the island, but has been the means of demonstrating to the world how yellow fever epidemics can be effectually suppressed. The results of Reed's and Carroll's investigations, and their practical application by the army almost excedes belief. They read more like a romance than actual facts.

If now malarial infections could be dealt with in the same forceful manner as has been done with yellow fever, it would not be long before it would disappear. This does not seem possible, because of its widespread distribution and of its clinical manifestations. If malarial attacks were as fatal as those of yellow fever, and were regarded as serious, probably more drastic measures would be taken against them, but so long as the great majority of cases are not fatal, there will be a disposition to temporize with them.

An anti-malarial campaign in a city is, comparatively speaking, less difficult than one in the country. In one the area is small, in the other of indefinite proportions. Whatever is done, is for the purpose of destroying the mosquito and preventing its breeding. This requires a large expenditure of money. Cities may be able to meet these expenses but the country districts can not, unless largely aided by the state or national governments. Any effective plan for eradicating malaria from a district of considerable area must, therefore, be considered as a public work. Many communities may possess the means of improving their sanitary condition by engineering, the destruction of mosquitoes and the control of infected persons, but such instances will be few and far between, when compared with the majority unable to do so. Even were it possible to rid malaria from a given place, it would be a long time before the danger of reinfection could be overcome. The rapid intercommunication between it and other places by sea and land is ever increasing, there is a constant shifting of the population, accessions to its population are constantly being made of these of immigrants from the malarial districts of Europe—many of these are infected—all these tend to perpetuate the parasite. And so far we have been unable to abolish the mosquito. We, therefore, naturally hesitate in advocating radical measures or hope for speedy success, when we stop to consider that an anti-malarial campaign must include fifty million of our population and over a million and a half square miles of territory.

The control of malaria, that is to say, to reduce the number of infections to a minimum, will perhaps be accomplished within a generation. It will demand a constant agitation of the subject, an education of the masses, and more in particular our legislators, before we can hope to accomplish what we desire. If the legislative bodies of state and nation could be impressed with the importance of improving the health of our people, and make available for this purpose, even a small amount of the sums annually expended on protecting the health of the live stock, we feel sure that before many years there would be a wonderful improvement in those districts now infested with malaria—a healthier people, an increased population, and more intelligent and energetic citizens.

We have as a nation been prodigal with our expenditures for developing our rivers and harbors, for promoting commerce. There has been expended on the Mississippi and Missouri Rivers alone, not to speak of the smaller, enough money to have constructed them anew, and to-day they are practically useless. If only a few annual appropriations could be diverted from these, and be expended in the improvement of the sanitary condition of the country, in the drainage of malarial swamps, it would have given a greater return than all that has been wasted under the guise of promoting commerce.

In this connection, another thought suggests itself, that is the employment of the idle labor in this public improvement. There are now many thousand able-bodied men wandering over these United States, having no settled abode, no employment, and seeking none. The tramp does not starve, some one, therefore, must feed and clothe him and care for him when sick. Now, in view of the fact the public care for him, he should be compelled in return to render a quid pro quo for his maintenance. No better plan could be suggested than to put him to work digging ditches, draining swamps and eradicating mosquitoes. The inmates of our penal institutions should be utilized in the same manner. These are cared for at the expense of the taxpayer, and many times by those who live in a malarial district. The employment of this class of persons on such public works would certainly be more humane and less cruel than to pen them up within four walls and keep them in idleness.

WHAT DOES A UNIVERSITY DO FOR A MEDICAL SCHOOL?

By Douglass W. Montgomery, M. D., Professor of Diseases of the Skin, University of California.

A university does much for a medical school. It gives it the university spirit, and the university ideals are what lift a medical school out of being a mere business institution into something that is far better.

They were idealists who built up the German universities after the Napoleonic wars. Teaching was of importance, but finding out things was of still greater importance, and they impressed their pupils with the value of investigation. For years they worked unknown, but after a time they began, as Carlyle put it, to tell the world what time it was. The work they did to discover the limits of our knowledge and to fill up its vacancies was found to be of the highest commercial value, and the English are now, on all sides, blaming their own universities for remaining as high schools and schools for gentlemen, instead of following the example of the German universities as the Americans have done. Medicine was early given a place in the German universities as affording a particularly advantageous ground from which to study the position of man in his environment. This was at a time when the highest ideal of our universities was to give a varnish of culture to boys who were, later on, to follow law or medicine, or who would enter the church, or teach. No grown man could enter our universities for purposes of investigation, or to satisfy his intellectual needs, any more than he could enter a high school. The universities of all English-speaking countries, a few years ago, thought it beneath their dignity to admit in their course any technical study whatever.

Several years ago I had a conversation with President Louden, of Toronto University, on this point. He said that at first he had been opposed to the entrance of a medical school, but had changed his mind. His point was that it strengthened the university. It increased the alumni, and the medical alumni were found to have more interest in the university than almost any other. It arose, he said, from the fact that the work of the medical graduates remained always in line with the work of their professors. Any investigation in medicine in the University was received with the liveliest interest and appreciation by a large number of alumni. On the other hand, those graduates who became lawyers, for example, cared little who taught Cicero or Roman law in the University.

To illustrate further the point of view of the Germans: Professor Winkel, of Munich, a few years ago, visited this Coast. The department of the University of California that interested him, and which he determined to have copied on his return home, was the University Dental School. At the time of Professor Winkel's visit, the medical school was struggling along on insufficient means, and its appearance was not imposing. One of our former professors, the late Dr. Hugo Toland, had given the University a building, and the faculty had steadily appropriated the money received from the students as fees to the betterment of the plant. The faculty was doing its work well, and we had, under Dr. H. A. L. Ryfkogel's direction, the best clinical laboratory then on the Coast. We later got a finer building from the State, and with the generous aid of Mrs. Phoebe Hearst, a pathologic laboratory with a salaried professor in charge, was established. This last might be called the first step in elevating the institution from a mere business school into being a university school. It gave it a chair which is in a peculiar sense a university chair, as one of the prime conditions of its life is investigation. It gave it a chair, the incumbent of which was not hampered by having to make a living by the practice of medicine. The next step was a department of anatomy established on a university basis, and the next was the call of Jacques Loeb to the chair on physiology and the generous gifts of Rudolph Spreckels and Dr. Morris Herzstein to build a laboratory and outfit it. Last year, while visiting the Medical Department of Cornell University, in New York City, some of the professors there admitted that even the millions spent by Colonel Payne had failed to get as broad a foundation as had been obtained on much less money by President Wheeler, of the University of California. The spirit of scientific investigation, what may be called the modern university spirit, can only be obtained through men who have it. The furtherance of medicine beyond mere technical excellence in its application depends on having men who have the spirit of investigation, the learning, the leisure, and also the plant to work with.

The fostering of this university spirit is what the university is doing for the medical school, and it is an admirable work and will be found eventually to be most remunerative.

On the other hand, let us see what a medical department does for a university. No modern school, not even a young ladies' academy, is without its course in natural history. It is the spirit of the age to study the workings of nature. The University of California has for years, under the Le Contes and others, laboriously built up its natural history and chemistry departments. Now the study of medicine is only a question in natural history. It is the study of man in his environment,—what will modify his health, what will restore it when lost, and under what conditions he can best live. Medicine is not only a question in natural history, but the most important of all of them.

These universities that give natural history courses alone miss the point and stimulus afforded by medicine, and are apt to become nerveless and inactive in these branches. The professors, after seeing a comparatively empty B. S. degree given to their pupils, see them go elsewhere and get their M. D. degree and become alumni, and closely knit ones, as above shown, not to the university where they received their primary training, but to the one where they got the degree that marks them out for all their subsequent life of usefulness and influence. This is what the great universities of Germany and America have seen, and what they felt themselves forced sooner or later to take advantage of. It is not a matter of choice, it is compulsion, the trend of the spirit of the age. It is the same compulsive force that drives the department of medicine up to being the chief department of every university of any size where it is introduced. It marks the distinction between the old scholastic university and the modern one.

Another reason why a medical department is so advantageous to a university is that it is to a large extent self-supporting. They do so well in this respect that even some proprietary schools, though wholly dependent on their fees for support, make a very creditable showing. A further reason why a medical department is advantageous is that it is a department that universally attracts to itself gifts. As previously indicated this has begun here, and that no department is likelier to receive donations is the experience of the other large universities of the United States. It is so with Columbia, Harvard, Johns Hopkins, and Pennsylvania. Just as soon as the funds of a medical school are in the hands of a conservative body of men, who are independent of the teaching body, so guaranteeing judicious and unselfish management, endowments are forthcoming. Nothing appeals more to the charitably-minded than medicine, for they see in it the alleviation of suffering; nothing appeals more to the broad-minded and cultured, for they see in the study of medicine the hope for the advance of the race. Our large cities would be heaps of putrescent filth, incapable of growing because of their enormous death rate, if it had not been for the discoveries in hygienic medicine. The real conquest of the tropics by the white races depends on hygienic medicine. Almost all the work on hygienic medicine is done either under the auspices of the government or in the universities of Germany and the United States.

In the above it has been striven to

show some of the reciprocal advantages which a medical school and a university render to each other.

There may be disadvantages, but I can not see them, and yet I do not think I am like Nelson at Copenhagen, who when asked to observe that the signal for retreat was run up on the Admiral's ship, put his spy-glass to his blind eye and remarked that he could not make it out.

ACUTE RADIATING PARENCHYMATOUS KERATITIS.

By Dr. Robert D. Cohn, Oculist to Mount Zion Hospital.

Read before San Francisco County Medical Society, August 6, 1903.

At the Santa Barbara session of the State Medical Society this spring I read the following brief report of a case coming under my observation: Mrs. X., aged 24, married one and one-half years, came to my office on Nov. 29, 1902, very much alarmed over a film that had suddenly come over her left eye ten days previously. Both eyes had been perfectly clear on her retiring, on her arising in the morning she was terrified to find the sight in her left eye almost gone. On looking into a mirror, she discovered the eye covered with a dense haze. Since her twelfth year she had worn glasses for myopia, was now wearing R and L-6, 5 sph.-1, 75 cyl. axis obl., otherwise her sight had always been healthy. She knew of no trauma, had not knocked her head against the bed, and, believing herself in perfect health, was at a loss to explain the cause of the trouble. On examining the eye, I found the cornea almost entirely occupied by a very remarkable infiltration. In the center was a grayish, homogeneous disc, about 4 mm. in diameter, from the periphery of which radiated twenty odd spokes or streamers, these latter extending to within a few millimeters of the limbus. The whole figure resembled more or less a flower, such as a sunflower, and was situated entirely within the corneal parenchyma. Closer examination broke up the disc into a mass of minute points, which showed the streamers to be distinctly striated, that is, made up of a string of dots, set end to end. In most of the streamers the innermost dot was very large and opaque, so that the disc was bounded by a chain of sharply-defined white spots, which were the most prominent infiltration foci of the whole figure. The anterior surface of the cornea showed no erosions, was smooth and bright, nor was there any deposit on its posterior surface. There was, moreover, an almost entire absence of all inflammatory symptoms, very little conjunctival irritation, no corneal vascularization, no iritis, no sensitiveness of the eyeball to touch, tension normal, slight photophobia, vision was limited to fingers in about one meter. Questioned as to previous specific infection (that being the etiology which would first suggest itself), the patient answered with a most vigorous and persistent denial. Both she and her husband appreciated the gravity of the case, and being correspondingly alarmed, insisted that if they knew of it, they would, in the patient's interest, most certainly not hesitate at an admission. In spite of this, I subjected the patient to a specific course of treatment: 45 inunctions of 4 grs. each, one daily, omitting every seventh day, together with iodid of potash, 10 grs., 3 a day, locally atropin, yellow ointment and warm applications. At the end of about the twenty-fifth inunction, the infiltration as a whole began to appear fainter; during the following days the streamers as well as the disc began to fade away, and at the end of the forty-fifth, the cornea was entirely clear.

It would thus appear, ex juvantibus, that, however strange the configuration of the infiltration, this was a case of specific parenchymatous keratitis; the treatment in its positive results would most certainly point that way. And yet I am inclined to doubt very strongly this specific etiology. Perhaps the strongest pair of restorative agents in fresh deposits of whatever nature and whether occurring in the eye or elsewhere, are mercury and iodid of potash, and the fact of their acting with such promptness in our case by no means proves the deposit to have been a specific product. General objective examination of the patient could discover nothing whatever to aid the diagnosis of either an acquired infection or of congenital disease, nor did anything in the patient's history, as we have already stated, give either of these assumptions any support. The latter, the negative history, it is true, does not count for so much,—syphilis insontium, syphilis of the unknowing, is frequent enough. What leads me chiefly to doubt its specific origin is the suddenness of both its appearance and disappearance. Even admitting that the beginning of the infiltration might possibly have been detected at an earlier date than the morning on which the patient first discovered it, the full flower burst into bloom overnight; the patient is positive she saw with that eye the night before, a few hours later she did not. Moreover, the effect of the treatment was surprisingly prompt. I need only to quote Weeks Posey and Wright's treatise. "In the average case the disease runs its course in from five to ten months, seldom less than five months even in mild cases. In the severest cases the clearing up of the cornea does not reach the maximum in less than two or three years." Assuming then that the case is not one of keratitis parenchymatosa specifica, it appears to me to resemble in a general way those of the group which Fuchs collects under the name of keratitis profunda, of which he says: "Gradually there develops in the cornea, generally in the center, a gray film, consisting of minute points or spots or of intercrossing streaks. After persisting for several weeks it gradually disappears without ulceration, no vascularization, or very little; inflammatory irritation sometimes slight, sometimes severe. The disease affects only adults, its term is four to eight weeks or more. Its origin is, in the great majority of cases, unknown. The three causes that have been advanced are: (1) Cold. These cases, which Arlt describes as keratitis rheumatica, are accompanied by intense inflammatory symptoms, pain and photophobia. (2) Malaria. These cases are often protracted and without inflammatory symptoms. (c) Traumatism, especially contusions. These cases run a rapid course toward recovery." So much for Fuchs. None of these etiologies appear applicable to our case, however, and we shall have to be content with assuming that it is not a case of specific parenchymatous keratitis, but resembles in a general way Fuchs' keratitis profunda, and is of unknown origin.

I have taken the liberty of reporting the case chiefly because of the remarkable configuration of the infiltration. While one or more of the members present may have seen it, it is the first in the experience of colleagues to whom I have shown it as well as in my own, nor have I been able to find it described in ophthalmic literature, through which it is true, I but briefly glanced, nor in the text-book of Fuchs, Vossius, Michel, Schmidt-Rimple, Wellway, Noyes, de Schweinitz, or Posey (Weeks). It is easy to understand anatomically the development of a stellate figure in the lens; how it arises in the cornea I am at a loss to explain. So much for the report I read before the State Society.

I have brought the patient here this evening because in June, several months after the complete disappearance of the disease in the left eye, precisely the same trouble began in the right eye. I am fortunate enough therefore to be able to demonstrate in the right eye to-night the same remarkable infiltration that I described in the above report. I have a little to add to that report. The only point of difference to which I wish to refer is this: Whereas in the first eye the infiltration appeared very suddenly, in the eye now affected its development was very slow. The patient came to me fearing she was about to have a repetition of the trouble that she had had in the first eye. I could detect nothing. In the course of the following week or ten days I could watch its gradual development. The patient is receiving the same treatment I gave the other eye and I consider the prognosis good. I have not brought the case to your notice on account of the doubt as to its specific origin, but on account of the unusual configuration of the infiltration, which is more or less in the nature of a rarity.

ASTHMA PRODUCED BY URETHRAL IRRITATION AND STRICTURE.

By G. L. Eaton, M. D.

It is not my intention to go into the etiology and histology of asthma in its relation to the reflexes, nor is it my intention to cite the different authorities and publications that have appeared in medical literature during the last ten or fifteen years, but suffice to say that asthma is, no doubt, very often of reflex origin. Dr. Page, of New York, claims that asthma requires for its production three factors: (1) A sensitive area of mucous membrane; (2) an abnormally sensitive nerve-center; and (3) an external irritant. Given all three of these factors simultaneously, an attack of asthma is produced, and it can not be produced by any two without the third. The sensitive area of mucous membrane may be situated in the naso-pharynx, bronchial tubes, stomach, or urogenital tracts. Such being the case, I here cite the history and treatment of four cases of urethral stricture and neurosis:—

Case 1. Mr. R. P. L., aged 28, has, up to the past three years, been in perfect health, with the exception of the minor diseases of childhood, such as whooping-cough, measles, and mumps, which he had contracted before the age of ten. Between the ages of ten and twenty-four, patient never had an unwell day. At the age of twenty-four, patient contracted an acute gonorrhea, which continued for eight months. During this time he consulted several physicians, who, apparently, did him but little good. He then became discouraged, neglected himself, decided that he would never be well, and continued to indulge in intoxicating liquors and intercourse. This apparently had but little effect upon his urogenital ailment, neither making it better nor worse. This course of debauchery continued for about six months, when patient contracted another gonorrheal attack, which has remained in a subacute stage up to the present time, Jan. 5, 1902. On Nov. 20, 1901, patient showed manifestations of gonorrheal rheumatism in the right knee. His present condition, upon examination, is as follows: Microscopical examination of the urethral discharge negative, with the exception of pus-cells and a few urethral epithelia. Urethral examination shows, for about five centimeters, a stricture admitting a No. 8 sound, American; very consistent in nature. First urine cloudy and full of tripperfaden. Patient's constitutional examination as follows: Good deal of gaseous pain in bowels; complains of nervous attacks, and is unable to sleep; has frequent attacks of testicular neuralgia, aggravated by asthmatic paroxysms, which last for three or four hours at a time. I advised urethrotomy for stricture, so that deep urethral examination and medication could be done. Under direct observation through the urethroscope, and the aid of cocain, I cut the stricture, and stretched the same to a No. 20 American sound with the Otis urethrometer, the hemorrhage being controlled by the extract of suprarenal capsule. By the aid of sounds, which were passed every other day, the patient made an uneventful recovery, and I was able to pass a No. 17 or 18 American sound with impunity. It is interesting to note that the testicular neuralgia soon disappeared, also flatulency and gastric disturbance; and the most remarkable was the disappearance of the asthmatic attacks, which, previous to the urethrotomy, were very frequent.

Case 2. Mr. M. R., tower-tender, aged 45. History of many attacks of gonorrhea and a constant sufferer from asthma. He is unable to lie in a recumbent posture, and, at times, becomes very cyanotic; has become accustomed to the use of amyl nitrite for the relief of the paroxysms. The purpose of consulting me was the inability to urinate, owing to a very contracted stricture of the urethra, about two centimeters from meatus. The same would not admit a No. 2 sound, American, and it was necessary for me to resort to the filiform bougie. After passing a number of the smallest, I succeeded in introducing a grooved director, and, after a thorough examination of the anterior urethra, I made several incisions through the stricture, and dilated same to a No. 17 sound, American. I noted at the time of instrumentation that patient's asthma became greatly aggravated, but after the dilatation ceased, the asthma ceased also. I treated the case by sound for several months, and then lost sight of him, but suffice to say that he became free from asthma as long as the stricture remained under treatment.

Case 3. Charlie R., aged 14, has had asthma for three years in a mild form, but perceptibly enough to cause mother and father great uneasiness. Upon examination I found that the boy had a congenital stricture of the meatus, which would not admit a No. 9 sound, French. He always had great difficulty in urinating, the same being very annoying, as the prepuce and glans penis were constantly in an excoriated condition. Under cocain, I performed meatotomy, so that the meatus would admit a No. 17 sound, French. The case went on to uninterrupted recovery, with complete disappearance of all asthmatic tendencies and attacks.

Case 4. ———, aged 30, whose history is free from any constitutional or functional disorders of the thoracic region, consulted me, on June 30, 1903, for stricture of the urethra. The history as elicited from the patient was as follows: Contracted gonorrhea about seven years ago, which was rather obstinate, as far as treatment was concerned, the same lasting for many months, and, presumably, years, as his present condition indicates. He further states that, a few months after the infection, he became a sufferer from asthma, which continued up to a few months ago. Careful examination of the nose and throat has been made by eminent specialists, in hopes that some irritable mucous membrane might therein be found, but all the examinations were negative. Upon examination, I found a slight urethral discharge, and, from microscopical examination, epithelia, pus-cells, and gonococci. The first urine was cloudy, and contained many tripperfaden; second urine clear. Examination of prostate gland and seminal vesicles showed that the right lobe of the gland was rather sensitive and slightly enlarged; seminal vesicles were normal, also their appendages. Urethral examination was very painful, owing to hyperesthesia, so much so that cocain was resorted to, and a No. 8 sound, American, was passed with some difficulty, owing to the multiple strictures, follicular and periurethral inflammation. At the time of urethral instrumentation, the patient would suffer from paroxysmal attacks of asthma, and, invariably, the night following the treatment the attacks would be aggravated, so much so that sleep would be impossible. After intercourse he was much freer from asthma than at any other time. I continued the following treatment, which consisted of the introduction, under cocain, of sounds Nos. 9, 10, and 12, American, every other day, with slight urethral massage over the sound. The hyperesthesia gradually disappeared, and I was able to discontinue the local anesthetic. I continued to dilate the urethra, until I was able to introduce a No. 22 French urethroscope, and through the same I made thorough application to the diseased follicles by injecting through the ducts 10 per cent solution of silbamine. After a few such treatments, the patient noticed a marked improvement, the attacks becoming less frequent, so much so that I attribute the reflex to be located in the urethra. With such a decision, I decided to stretch the urethra to its full limit, which proved to be a No. 28 sound, French, at the same time making deep applications to the posterior urethra. From the above treatment I gained the most happy results. The asthma completely disappeared, and the patient is now enjoying perfect health.

In closing, I would suggest that a careful examination be made of the urethra, bladder, and ureters, as well as that of the nose and throat, in all asthmatic conditions; and I sincerely hope that this small contribution will be of some service in alleviating some unfortunates who are suffering from these painful affections.

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INFANT FEEDING.

By R. C. MacDonald, M. D.

In the Boston Medical and Surgical Journal of Nov. 21, 1901, I called attention to the fact that the then common methods of milk modification were theoretically erroneous, because they overlooked the marked deficiency of lact-albumen in cow's milk as compared to human milk. My article, using tables taken from "Foods and Dietics," Robert Hutchinson, London, 1900, stated that in the curd of human milk the hard, fibrinous caseinogen and the soft, flocculent albumen were equal; while in the curd of cow's milk there existed six parts of caseinogen to but one part of albumen. The figures for cow's milk are, probably, too high, four to one being more accurate, but the disproportion, in either case, was sufficiently startling.

Recently some efforts to overcome this deficiency of albumen, have been made by using whey as a diluent, and the most recent formulæ for modified milk enable the prescriber to regulate, to a certain extent, the proportions of caseinogen and albumen. This is a step in advance in making cow's milk more nearly like human milk; but, unfortunately, it adds another complication to an already over-complicated system. The percentage method of milk modification, with its algebraic formulæ and its complicated tables, is very beautiful in theory, and, no doubt, equally so in practice, when it can be applied; but, aside from its expense, the intricacy of its calculations must prevent its general adoption.

Striving toward the ideal does, in time, cause an elevation of the practical; but if the ideal is made too difficult of realization, the practical level is liable to be lowered, owing to the seeming impossibility of the task set. I would not wish to express the cynical thought that the percentage system of milk modification is such an ideal, but I fear it may be so. The net result of the system may well be to cause a division of the profession into three classes. A few physicians will use the method properly and get the best results; a fairly large number, aspiring to be considered scientific, will use it, more or less improperly, getting such results as might be expected from any improperly used, complicated system; the great bulk of the profession, recognizing the impossibility of carrying out the method, will become discouraged and go on using "any old thing" which may seem to fit the case; this condition is the more likely when we realize, as we must, that after modifying your milk, with all the complicated algebraic formulæ provided, the result, in its application to any individual case, is very greatly problematical; in fact, that the last analysis of any system of infant feeding is the purely practical question, Does the particular combination fit the particular case? It is, of course, only by averages that we can measure the value of any method, and I have no doubt that the scientific method of milk modification, if scientifically applied to a series of cases, will produce better general results than the empirical method. My objection is not to the method, but to its difficulty of application and to the fact that, because of its impracticability, some less purely scientific but more feasible method will be overlooked in the discouragement which follows on the failure to succeed with the more ideal system. A fairly scientific, practical system which can be easily applied will produce better general results than a more scientific method which can be used only in a limited number of special cases. Scientific milk modification has become such a "Shibboleth" that many physicians are afraid to admit (what is the fact) that they know very little about it; and many more are equally afraid to admit (what is equally a fact) that in feeding infants they "cut their coat to fit their cloth."

The artificial feeding of infants is an experiment, pure and simple; some will digest almost anything, even the much-abused starch, while others will pine away under the most scientific feeding; and it is impossible at the beginning for any one to say just what particular combination will suit any particular case. It may be freely admitted that, scientifically applied, the percentage system of milk modification ought to give the best averages; but that it is capable of general application, may be as freely denied. What is needed is a fairly scientific method which can be used, practically, in general work. No method will make impure milk pure, nor keep clean the utensils the mother or nurse is too lazy or too indifferent to attend to;

these and many other difficulties confront the physician before he has time to apply any system; overcoming them, as best he may, his object is to make cow's milk, the only practical substitute for human milk, as digestible as possible. By reducing and breaking up the hard casienogen with low percentage decoctions of cereals, by overcoming the deficiency of albumen, and also of animal phosphorous, with addition of finely triturated whole egg, by using milk sugar and an alkali, by varying the proportions of cream—it is quite possible to get a food which approximates very closely to mother's milk, and which will agree with the baby. No doubt some experimentation will have to be made; but in this we shall not differ from the most scientific of milk modifiers, for, after he has worked out his algebraic formula or figured his percentages from a table, he, too, must submit his combination to the crucial test of the infant's stomach and system. The success of any method depends on the judgment of the user, and for that reason the most essential ingredient in any mixture is brains. Given a method she can reasonably understand and intelligently use, the mother, or nurse, will put brains into her milk mixtures, and will get results, but with a method too complicated she will be forced to leave out the most essential ingredient, and the results will be what might be expected from a perfunctory service.

ILLUMINATING GAS.*

By E. V. Lamorce, M. D.

Read before the Saturday Night Club, August, 1903

The ordinary illuminating gas, which consists chiefly of light carburetted hydrogen, contains also vapors of volatile, liquid carburets of hydrogen, carbonic oxide, and other elements. Light carburetted hydrogen is in itself hardly poisonous, but the composite gas, which is now everywhere so freely used for burning, has frequently caused fatal accidents. Still the atmosphere may be very offensively loaded with it, and yet be breathed for a short time with impunity. It does not appear to act merely as an asphyxiating agent, but rather like a narcotic.

The danger arising from the inhalation of illuminating gas varies with the kind of gas and with the thoroughness of its purification; the principal danger arises from the carbonic oxide which the gas contains; ordinary illuminating gas made from bituminous coal and well purified consists chiefly of hydrogen (40 to 50 per cent), marsh gas (35 to 40 per cent), carbonic oxide (4½ to 7½ per cent), olefiant gas and other hydrocarbons (4 to 8 per cent), and usually very small amounts of carbonic acid gas and air. The so-called water gas, made by passing steam over anthracite coal heated to a white heat, yields a gas which burns with a colorless flame and contains 40 or 50 per cent of carbonic oxide; this is mixed with a highly luminous gas made from naphtha, petroleum, or cannel coal to give its illuminating properties, but even then it contains a very large percentage of carbonic oxide, which is one of the most active of poisons, producing when inhaled speedy death, and according to Leblanc, one volume of it diffused through one hundred volumes of air totally unfits it to sustain life; and it appears that the lamentable accidents which too frequently occur from burning charcoal or coke on braziers and chafing-dishes in closed rooms result from the poisonous effects of the small quantity of carbonic oxide which is produced and escapes combustion, since the amount of carbonic acid thus diffused through the air is not sufficient in many cases to account for the fatal result. When it was proposed to supply the "Invalides" in Paris with water gas, a commission was appointed consisting of Messrs. Dumas, Chevreual, and Regnault, eminent chemists, to investigate it. They found that it contained from 30 to 40 per cent of carbonic oxide, and reported, "that it would be dangerous to the occupants of the institution to introduce, even by way of experiment, gas obtained from the decomposition of water by the Kirkham process." This gas was the odorless carbonic oxide and hydrogen mixture. H. Letheby says: "Seligue, in 1840, obtained permission to use the gas in the towns of Dijon, Strasburg, Antwerp, and two of the faubourgs of Paris, and Lyons. At Strasburg an accident occurred which put a stop to its use. The gas escaped from the pipes into a baker's shop, and was fatal to several persons; and not long after an aeronaut named Delcourt, incautiously used the gas for inflating his balloon. He became insensible in the car, and those who approached to render him assistance fainted and fell likewise. The use of the gas has therefore been interdicted on the Continent.

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*106 ENDED LIVES WITH GAS THE PAST YEAR

According to the records of the Coroner's office covering the year ending June 30, 1903, there were 106 cases of accidental death from illuminating gas. Altogether 1,135 deaths were investigated. Of these, 197 were found to be suicides. Three hundred and thirty-three were accidents. Disease ended the lives of 518. During the year there were 22 murders and four cases of justifiable homicide.

Of the suicides, 26 were Germans and 34 were natives of this country. Eight Chinese took their own lives. Male suicides numbered 138; female, 59. Domestic troubles caused 20; drink, 15; financial embarrassment, 18; disappointment in love, 14; and sickness, 46. Seventy were married and 67 single. Three telephone operators voluntarily ended their careers.

Eighty-five persons suffered fatal falls, most of them from buildings in process of erection. Seventy-one died from shock following surgical operation. Fifty women and eight men smothered life's flame by taking carbolic acid.

The first symptoms are nausea, headache, noises in the ears, and great prostration. All of these become aggravated, the breathing then becomes oppressed, the limbs paralyzed, and death is preceded by coma and convulsions.

The post-mortem appearances are, generally, intense injection of the vessels and redness of the bronchical mucous membrane and of the lungs, and a dark color of the blood. In the fatal cases which occurred at Strasburg, and which are reported by M. Lourdes, the bronchial tubes were filled also with a white, thick, and viscid froth, streaked with blood. The following case is reported by Gartner, of Struttgart. The gas affected a lady, her servant maid, and also an English pointer dog. The lady was first seized; her illness began with an affection of the head, sickness, vomiting, and purging of thin rice-water like stools, in which whitish flakes were observed. After twelve hours she recovered, but felt very drowsy. On the fifth day she experienced pain in the back part of the head, lassitude, vertigo, tinitus aurium, and loss of appetite, accompanied by a loaded tongue, a small pulse of 90, and cessation of the menses. Blood of rather a dirty dark-red color presenting no buffy coat was abstracted from a vein. Next day the patient was worse: she was quite insensible, and lay with closed eyelids; the eyes were turned up, the pupils were much contracted, and unaffected by the light; the face was not swollen; there was trismus; the arms were flexed at the elbow-joints; the respiratory movements were very feeble; the pulse was hardly perceptible; the skin was warm, but insensible to the touch. She was, however, restored by venesection and other remedies. In the servant girl similar symptoms occurred, but not with much severity, which may be attributed to the fact that the atmosphere of her chamber had not been so strongly impregnated with the gas. She had severe cramps of the extremities, great jactation of the hands, flexion of the arms at the elbows, great restlessness and inclination to yawn. Her blood presented no buffy coat. Latterly she had a non-febrile, bloody diarrhea. She recovered in fourteen days from the date of her seizure. The dog was found insensible and quite stiff as if dead, but it soon recovered. A man employed to clean a covered passage for water into which a gas pipe had leaked several days before, was sickened by the smell of the air, and fell with his head under water. In about five minutes he was with difficulty dragged out by means of a noose around his neck. He was pale and breathed feebly, but gradually his warmth and pulse became natural; the breathing, however, was oppressed and guttural, and the muscles of the trunk and limbs spasmodically contracted. Death took place in about seven hours. The body, which was examined thirty-five hours afterwards, was rigid and everywhere seemed congested with blood. It exhaled a strong alliaceous odor, and the stomach and intestines were distended with a gas which took fire and burned when flame was applied to it.

Chemical examination: The detection of this form of poisoning after death depends chiefly upon the recognition of the carbonic oxide in the blood by the spectroscope, since the coloring matter of the blood unites with it to form a compound which gives characteristic absorption bands when examined by the spectroscope. In case of poisoning by carbonic oxide or any other gaseous mixture which contains it, the blood has the bright red, almost rose color, which remains several days. This color is due to the union of the blood pigment, hemoglobin, thus depriving blood pigment of one of its physiological functions, viz., that of carrying oxygen through the body. If blood which contains this compound of hemoglobin and carbonic oxide be examined after suitable dilution with water, with the spectroscope, two absorption bands similar to those of the natural blood pigment will be seen, but differing from them by the fact that, when treated with sulphide of ammonium, they do not disappear, or, rather, coalesce; moreover, this compound is a very stable one, and is not destroyed by drying, so that it can be detected, according to Eulenberg and Jaederholm, in blood after it has been dried for several weeks, by moistening with water and examining with the spectroscope.

EPITHELIOMA OF THE PENIS.

By Douglass W. Montgomery, M. D. Professor of Diseases of the Skin, University of California.

Read before the California Academy of Medicine August, 1903.

As Farquahr Curtis has remarked, epithelioma of the glans penis, and prepuce usually exhibits a low grade of malignancy.[1] In the following case, however, which I had the privelege of observing in the practice of Dr. George Chismore, the disease was distinguished by an unusually high degree of malignancy, and had also several other interesting features, which will be commented upon in the course of the article.

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[1] "It corresponds in etiology and prognosis with the epitheliomata of the skin observed elsewhere on the body."—B. Farquahr Curtis, M. D. An American Text-Book of Gento-Urinary Diseases, Syphilis, and Diseases of the Skin. Bangs-Hardaway.

The patient was a man, aged fifty-one, who had congenital malformation of the head of the penis. There was slight hypospadias, and there was no sulcus, the foreskin springing directly from the top of the corona. He said that formerly the foreskin had sprung from the glans about its middle, but had been torn back to the top of the corona when he was about twenty years of age. After so tearing back the prepuce, the surface of the glans healed and was perfectly smooth with no scar, but the junction of the glans and the prepuce always remained weak, and liable to crack and easily tore during connection. It had never struck me so forcibly before seeing this patient, how admirably the ditch or sulcus behind the corona with its rounded surfaces serves to strengthen the union between the mucous membrane of the prepuce and that of the glans. He went on to say that in 1875 he acquired a chancre, which unfortunately developed on this weak spot. The chancre was followed by phimosis, which lasted for three months. Then symptoms of early constitutional syphilis developed, such as sores on the palms and soles and a few pimples on the scalp. He got well under antisyphilitic treatment, which he continued for four years. After this incident the cracking and fissuring at the junction of the glands and prepuce became more marked.

I first saw the patient July 1, 1893, and he told me that eighteen months previously an ulcer had formed at this tender spot. At first a chancre, or at least a syphilitic sore, had been suspected, and he had taken as much as one hundred and fifty drops of iodid of potash a day with no effect. The lesion when I saw it was a circular, well circumscribed, prominent, button-like projection, with rounded edges, a soft, granular surface, and a depressed ulcerating center. The growth was fairly firm and bled easily when touched. There was no pain, and only at one point tenderness. There was a smaller button-like growth of the same character as that on the glans, situated on the inner surface of the prepuce, directly overlying the original growth. It looked as if the infection had sprung across the space by contact. There was a pea-sized lump in the skin on the right side of the body of the penis, which was freely movable, firm, and neither painful nor tender. There was one enlarged, slightly tender, oval lymphatic nodule in the right groin, which was immovably attached to the subjacent structures, but the skin over it was perfectly movable.

A bioscopy of the original growth confirmed the diagnosis. It was a papillo-epithelioma. The situation was explained to the patient. The cancer was a most rapidly growing one, and its high degree of malignancy was shown by its jumping across the prepucial space by contact, and by its early involvement of the lymphatic system, for there was no reasonable doubt that the enlarged lymphatic nodule in the right groin was cancerous, for if it had been enlarged simply from pyogenic infection it would not have been immovably fixed to the subjacent structures. The prognosis was therefore bad, no matter how extensive the operation for removal might be. Under the circumstances the patient declined an extensive operation, and wished the removal of the local affection only.

Dr. Chismore removed the prepuce and part of the glans, July 22, 1893. A short time afterwards, on August 5, 1893, the lump on the right side of the body of the penis was removed; it was firm, almost wooden to the touch, and of a yellowish color on section. It was also found to be cancerous. On January 5, 1894, the sites of both operations looked well, but a pea-sized nodule had developed beneath the skin behind the glans, and there were two lymphatic nodules in the right groin the size of marbles. On February 3, 1894, Dr. Chismore removed the above-mentioned nodule behind the glans and several lymphatic nodules from the right groin, which were found infiltrated with epithelial cells. On August 6, 1894, Dr. E. G. McConnell removed a number of enlarged lymphatic nodules from both groins, which were also found epitheliomatous. These operations for the removal of the affected nodules were done, not with any hope of eradicating the disease, but simply to rid the patient of the diseased tissue which was inconveniencing him. The patient died in the beginning of April, 1895, with a large, stinking cauliflower growth in the right groin.

Cancer of the glans and prepuce is almost always preceded by phimosis; almost always congenital phimosis.[2] Next in frequency as a precancerous condition is the existence of a scar, usually one from a chancre or gumma.[3] Scar tissue itself, wherever found, as a tissue of low resistence, is more liable to cancer than normal tissue. Chronic irritation, here as elsewhere, has been looked upon as a cause of epithelioma, and it is thought that it is the long continued irritation by the retained secretions that causes cancer so frequently to occur in congenital phimosis. In this case there was a series of unfortunate accidents, first congenital phimosis, then a weak joint between the glans and the prepuce from absence of the sulcus; following that the implantation of a chancre directly on the weak joint; and all the time there was the increasing liability to irritation from retained secretions, which is always an especially marked feature of the glans and prepuce in advancing age.

The appearance of a secondary growth exactly like the first but smaller, and occurring on the inner surface of the prepuce exactly overlying the first growth is very interesting from a pathologic standpoint. It could not have been a metastasis by way of the lymphatics, as the lymphatic supply for both situations is not the same, and it was not a growth by continuity, as it was well separated from the primary growth by healthy tissue. One or two views might be taken of its origin, either it was the effect of irritation of the original growth and its secretions on the overlying mucous membrane, or it was an implantation of cancer tissue. It is hardly probable that it was due to irritation, because the original tumor had not existed long enough to cause a cancer by irritation alone. The view that it was due to implantation seems more likely to be true, and is directly in line with the experimental work done in this direction by Ribbart and Leo Leob.[4]

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[2] A. Marmaduke Shield says: "In exceptional, very exceptional, cases, cancer may attack a non-phimosed penis. I know of such cases in literature, but have only seen one. Here the cancer sprouted from the scar of an imperfectly-healed chronic syphilitic sore."—The Lancet, Jan. 13, 1900.

[3] Demarquay (quoted by R. W. Taylor, Venereal Diseases, 1895, p. 443) considered that syphilis itself, as syphilis, and not merely because of its resultant scar tissue, is an ethiologic factor in the occurrence of cancer of the penis.

Such clinical instances of implantation are very rare, as occuring between mucous membranes or on the skin. A sharp distinction must be made between these and instances where malignant disease jumps across the abdominal cavity or any other lymph or serous space. The abdominal cavity is a serous sack, a lymphatic cavity, and malignant disease jumping across it, as from the ovary to the abdominal wall, is more comparable to the passage of malignant disease along the course of the lymphatics than to implantation on a surface covered with stratified epithelium.[5]

In this case, as before stated, the several kinds of operative procedure were laid before the patient. There was simply the eradication of the foci of disease as they existed in the penis; there was also this together with the clearing out of the lymphatics of the groins; or there was complete amputation of the privates with the clearing out of the lymphatics of the groins. He was told that the prognosis in any case was not good, for the disease had shown itself to be very malignant. The occurrence of a cancer opposite the original cancer, the appearance of a nodule under the skin over the body of the penis, the early implication of the lymphatics in the groin, and the immovable fixture of one of the affected groin lymphatic nodules all pointed to extreme malignancy. The cancer itself was soft and cushion-like, which also indicated high malignancy, for the more chronic and horny the cancer, the less likely are the lymphatic nodules to become involved. There was also to be considered the feeling of moral degradation and the continued great annoyance and pain in amputation of the privates. As it turned out, amputation of the penis would have been useless, as nothing occurred in the organ that was not easily eradicable by simple measures, and none of the men who were interested in the case believe that the radical operation would have diminished the amount of pain or distress suffered by the patient, or would have deferred the fatal termination.

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[4] On Transplantation of Tumors, by Leo Leob, M. D., American Medicine, Vol. V, No. 11, p. 412. These experiments were made by the implantation of sarcomas in rats.

[5] Last year, Aug. 6, 1902, another case where cancer appeared to have jumped across from one mucous membrane to another came to our notice. This patient was a man, 64 years of age, with an epithelioma of three months' duration on the right side of the tongue two inches behind the tip. It was very hard with indurated borders overhanging its slightly punched out center. It had started as a pimple, and had spread peripherally. There were shooting pains in the tongue, and pains in the right temple, and the patient had not eaten solid food for months. There was a lesion on the soft palate of later origin than that on the tongue, and directly opposite to it, which was noted at the time of seeing the patient as being probably an instance of autoinoculation. On learning the true nature of his malady the patient refused operation, and went home to die.

DEPARTMENTS

DISEASES OF CHEST.

By Albert Abrams, M. D.

Diastolic Murmurs without Lesions of the Aortic or Pulmonary Valves.—Cabot and Locke refer to diastolic murmurs without organic lesion of the valves as not infrequent in localized or diffused dilation of the aorta. Adhesions between the pleura and pericardium may likewise cause such murmurs which are notably affected by posture and respiration and are caused, no doubt, in most cases, by suction or pulsion exerted by the heart upon lung areas adherent to the pericardium (cardio-respiratory murmurs). Diastolic murmurs are likewise heard in intense anemia, when the erythrocytes are reduced to or below 1,000,000 per cub. mil. In such severe forms of anemia, the murmurs can not be explained as cardio-respiratory murmurs nor to permanent dilation of the aortic ring, and they are not due to diastolic accentuation of a venous hum.—Johns Hopkins Bulletin, May, 1903.

Osler's Disease.—Osler describes a definite clinical polycythemia and enlarged spleen. The cyanosis is most marked about the hands and face, the skin of the entire body being a dusky hue. The cyanosis is more intense in cold weather, and is aggravated by any existing bronchial catarrh. It may lessen on bright, clear days. The cyanosis may extend over a period of from three to ten years. While constant, it varies greatly in intensity. No respiratory distress is associated with cyanosis. Close examination of the skin may show many fine dilated venules. The blood shows increased viscidity. The extraordinary polycythemia is a special feature. In Cabot's case, the maximum blood count was 12,000,000 per cmm. In eight cases the count was 9,000,000 per cmm. The hemoglobin percentage was high, ranging in one case to 165; the usual range was from 120 to 150. The specific gravity of the blood in one case was 1080; in another, 1067-1083. The leucocyte count in the majority of the cases was below 10,000 per cmm. The spleen was enlarged in seven out of the nine cases. The urine, in seven of the cases, showed a trace of albumen, with hyaline and sometimes granular casts. The symptoms were varied. Headaches, weakness, and prostration, were common. No pyrexia in any case. The pulse was of high tension and the vessels sclerotic. The torpor, mental, and physical: the sensation of fulness in the head, with headache, vertigo and in some cases nausea and vomiting, correspond with the symptoms to which mountain climbers and aeronauts are subject.—American Journal of the Medical Sciences, Aug., 1903.

The Etiology of Endocarditis in Childhood.—S. Blum, after referring to congenital endocarditis, which may establish a condition predisposing to attack of endocarditis, passes on to a consideration of acquired endocarditis. Rheumatism, as in adults, is associated with the greatest number of cases, but the proportion is far greater than in adults. Two reasons are assigned for this predisposition, viz., the more delicate organization of the endocardium with a diminished power of resistance. Endocarditis may precede the joint symptoms; in fact, it may be the only manifestation of the disease. Chorea is not an infrequent concomitant of endocarditis, and the occurrence of tonsillitis as an etiologic factor must not be disregarded. All the acute infectious diseases may be the initial factors; here two factors operate; a nonresisting endocardium, made so by the disease which makes unusual demands on the heart, the fever and noxious substances in the blood, and chiefly the action of specific micro-organisms. The author in referring to experimental endocarditis, notes the observations of Winge and Heilberg, who failed to cause endocarditis by introducing bacteritic embolic masses into the blood of rabbits, but like experiments of others who made a point of mechanically wounding the cardiac valves succeeded in causing endocarditis with diverse micro-organisms. His own investigations embrace positive results with strepto and staphylococci, tubercle bacilli, pneumococci, typhoid bacilli, and the bacillus pyocyaneus. Bacterial endocarditis then does occur, but a locus minoris resistentiæ must be present. Malignant endocarditis is exceptional in infancy and rarer in late childhood, and when it does occur it is most frequently associated with rheumatism. From the atheromatous changes peculiar to senescence, the child completely escapes, but this immunity is more than counterbalanced by the exanthemata from which children are the chief sufferers.—Archives of Bediatrics, May, 1903.

GENITO-URINARY DISEASES AND SYPHILIS.

By G. L. Eaton, M. D.

Epispadias.—J. B. Bullitt, M. D., in an article discussed at length the different operations that have been in vogue for the eradication of this lack on nature's part; dealing especially with Cantwell's method, that is the transplantation of the new formed urethra, which he constructs in the following manner: A dissection of the urethral mucous membrane and then transplanting it beneath the body of the penis, permitting a quick result to be obtained, which relieves entirely or in a great degree the incontinence. The operation as described is made as follows: A curved pair of forceps are first introduced into the bladder and its point made to depress the perineum. This is cut down on and the drainage of the bladder provided by means of a rubber tube which is left in place until the process of healing is complete. Next, an incision is made on both sides of the penis at the junction of the mucous membrane on the skin, beginning at the opening of the bladder and extending distally to the end of the glands penis. The mucous membrane lining the groove on the dorsum of the penis and marked out by these incisions, is now dissected up; when this is thoroughly done, the urethra can be drawn out to double its former length. This mucous membrane flap is now converted into a urethral tube by suturing the free edges together, and should be long enough to reach quite to the end of the glans penis. Next, a new bed for the urethra is made by dissecting apart the two cavernous bodies, leaving them attached only by the skin at the under surface of the penis. The urethra is now transplanted into this groove between the corpora cavernosa; a few sutures serve to anchor the new uretha to the skin. The corpora cavernosa are brought together and held in apposition by sutures, and then the overlying skin covers all in a very neat manner as it is very abundant. The author utilizes this redundant skin in making the anterior urethra, if there is not sufficient mucous membrane to extend to the glands penis. Journal American Medical Association, Aug. 1, 1903.

Malignant Tumors of the Testicles—according to Coley, are very likely to be sarcomatous. They also, contrary to Butlin, are not infrequent in young adult life, between twenty and thirty. As a cause, trauma plays a very prominent part. The diagnosis is not difficult; tuberculosis of the organ most closely simulates the condition. In sarcoma the tumor is of a uniform consistence, the reverse of which is the case in tuberculosis, the epididymis being harder than the testis proper. Coley does not agree with Kober as to the favorable outcome of these cases, but admits that most of those he has seen were not in the early stage, where treatment might be expected to be most effective. The rapid course of the disease is somewhat notable, sarcoma being more early fatal here than in other parts, as a rule. The treatment, he holds, consists in the complete removal of the testis and cord as high up as the internal ring, as soon as the diagnosis is arrived at. In addition he advises the admistration of mixed toxin serum. The prognosis of teratoma is good, though recurrences and metastases do sometimes occur.—Medical News, July 25.

Sacculated Bladder.—Sacculated bladder following injury or localized disease is rare and of short duration, which is soon followed by a rupture of the wall, or a cure by cicatrix. It is characterized by numerous depressions in the vesical wall, varying in size from a pin's head to a cavity large enough to contain the urine of a normal bladder or a stone several ounces in weight. Any part of the circumference of the bladder may be affected and may contain numerous deposits as well as urine. As the condition increases, the opening of these sacculi become smaller, and after a time on account of the unsatisfactory emptying of the pockets the cavity is enlarged and residual urine is contained in each sac. The primary cause of this condition is usually a hypertrophied prostate gland, or it may be a stricture of the urethra. This obstruction causes a strain to the bladder muscle, and after a time fractures of the connective tissue take place leaving slight depressions which gradually grow larger forming sacs or cavities. It is difficult at time to discover even calculi of large size and weight which lie concealed in the tissues, the opening being to detect them. Cases of sacculated bladder make up nearly 80 per cent of all cases of diseased bladder with hypertrophied prostate, or following tight urethral strictures in persons of mature years. Unless the sacculi are very numerous or extensive ulceration has taken place, there are few symptoms referable to this condition. There will be found great difficulty to completely wash out such a bladder. Symptoms of pain following an irrigation of a diseased bladder may be referred to this condition of the organ, which might be caused by washing out the bladder with a moderate amount of force, which in an ordinary bladder would be of no special importance. Constant irrigation of a bladder whose walls contain small or large calculi, instead of relieving the symptoms often set up a severe and active inflammation. The diagnosis of this condition is rather difficult, which must be made by exclusion, aside from positive information obtained by a cystoscope, if it can be used, which gives positive evidence of this condition. The treatment consists in an early and complete removal of the cause, the cystitis being much relieved by the drainage made by and following the operation for this purpose. Curettage through a supra-pubic opening is not to be thought of. Cystitis in such a bladder is absolutely impossible of cure. Drainage of the bladder can not drain these pockets. Fortunately these cases do not as a rule give any very severe special symptoms. The bladder frequently seems to tolerate them without giving any special symptoms calling for treatment. This condition offers an explanation for those cases of cystitis which no amount of irrigation will wash free of pus; for the severe pain produced in an organ previously quiet, and for the rapid recurrence and apparent disappearance of stone which had been previously found at an earlier examination.—International Journal of Surgery, Sept., 1903.

Cystotomy in Chronic Cystitis.—The following procedure has given gratifying results in cases where satisfaction has not been by the use of drugs and injections. The majority of cases in the female can be satisfactorily treated under atmospheric distention. The exceptional cases may be treated as in men, which is a follows: The bladder is distended with twelve to fourteen ounces of a 4 per cent boric acid solution, and a male sound introduced. Trendelenburg position. Incision to the symphysis pubis, cutting through the fat in the prevesical space down to the bladder, which is pushed into view. Being caught with volsella forceps, an incision one and a half to two inches is made extending toward pubis. Interrupted stitches of cat-gut should be inserted to prevent the wall from retracting. After absorption of liquid with gauze sponges, such applications as may be necessary should be made, followed by irrigation with 4 per cent boric acid solution at a temperature of 110° F. The upper angle of the wound is closed by provisional silk-worm gut sutures, bladder being left open with gauze drainage to be removed when saturated and replaced by new. Irrigate each day with 4 per cent boracic acid solution after the first forty-eight hours. In from five to ten days patient voluntarily urinates. Permanent closure generally from three to four weeks with gratifying results.

SURGERY AND ANATOMY.

By Wallace I. Terry, M. D., and C. G. Levinson, M. D., San Francisco, Cal.

Surgery of the Pancreas.—In Mikulicz's address before the last Congress of American Physicians and Surgeons, the subject of surgery of the pancreas was handled by him in a masterly manner. His experience in that particular line embraced sixty cases. He divides the methods by which the pancreas may be exposed into the transperitoneal and the retroperitoneal—the first giving us more complete access to the organ. The difficulties to be met with are numerous —the danger of injury to neighboring organs and large blood vessels,—the control of hemorrhage from the pancreas itself, the leakage of pancreatic secretion and subsequent fat necrosis and changes in the peritoneum, all these, plus especially the difficulty of diagnosis, have hindered the advance in surgery of the pancreas. Mikulicz believes that drainage is necessary whenever pancreatic tissue has been exposed. The recorded cases of injury to the pancreas are still few in number and experience goes to prove that severe injuries to the pancreas, which are not submitted to operation, terminate fatally almost without exception. Of those operated upon 72 per cent recovered. Regarding pancreatitis, Mikulicz prefers the classification of Robson into acute, subacute, and chronic. The acute form he considers as a phlegmon of the pancreas and advises deep incisions and drainage of the gland —a procedure that was successfully employed by Porter of Boston. The method of dealing with the subacute and chronic forms will depend upon a number of conditions; whether an abscess is formed, whether the inflamed pancreas is obstructing the common gall-duct, etc.—Annals of Surgery, July, 1903.

A Review of Three Hundred and Three Operations upon the Stomach and First Part of the Duodenum.— It is not the lot of many surgeons to have such a large experience in the surgery of the stomach and duodenum as has fallen to Wm. J. Mayo. His observations, while largely of a statistical nature, contain much of general interest, especially those referring to gastric ulcer and cancer. There were 28 operations upon the first portion of the duodenum, 18 for ulcers, with 2 deaths, and 10 for adhesions and fistula, with no deaths. Of the stomach operations, 173 upon 168 patients were for benign affections, with 11 deaths (6.5 per cent), and 112 upon 109 patients for cancer, with 17 deaths (15.6 per cent). The majority of operations were for thick-based, chronic ulcer of the stomach or its late results, and these cases were very satisfactory. In 2 cases, malignant degeneration of the margin of a chronic ulcer was demonstrated, and in 60 per cent of the malignant cases a previous history of ulcer was obtained. Mayo examined the pylorus in over 300 cases under anesthesia, and found that the normal pylorus will allow the thumb and forefinger to nicely meet,—about the caliber of a silver dime. Regarding cancer of the stomach, he believes in a radical operation whenever it is possible to carry it out. The hope of the future lies in early exploratory incision, and the necessity for this depends upon clinical observation rather than laboratory methods, which too often only become valuable when the extent of the disease is beyond cure. Given a patient of middle or later life, who begins to lose flesh and appetite and suffer from indigestion, without apparent cause, the possibility of cancer should be considered, and if the source of the symptoms can not be shown within a few weeks, the situation should be explained to the patient, and the choice between exploration and procrastination allowed him. Mayo's method of resection of the stomach is essentially the same as Robson's and Moynihan's,—all the lesser curvature and the greater curvature as far as and including the left gastro-epiploic artery; these lines include practically all the main lymphatic channels and glands. The duodenum is closed, and a loop of jejunum is anastomosed to the stump of the stomach, with an entero-anastomosis between the two limbs of jejunum. As regards gastro-enterostomy, he uses the posterior suture operation over the bone bobbin for benign obstructions, and the anterior method with the Murphy button for malignant disease. As between the suture and the Murphy button and the anterior and posterior he has no marked difference in results beyond the occasional retention of the button in the stomach. Gastro-enterostomy, if the pylorus be unobstructed, is far from satisfactory. In 28 cases of gastro-enterostomy with open pylorus, 8 came to secondary operation from contraction of the gastro-intestinal opening, while in all cases with permanent obstruction at the pylorus there were no cases of secondary operation from this cause. Possibly Finney's operation of gastro-duodenostomy will be the preferable one when the pylorus is open.—Annals of Surgery, July, 1903.

Deep-Seated Lipomata.—Dertinger in an article on the deep-seated lipomata calls attention to a number of interesting points in reference to the diagnosis of this condition. He mentions the following cases where mistakes in diagnosis have been made: 1. A tumor, which had developed within a year, was situated under the deltoid, and was accompanied by severe pain. Diagnosis: Soft sarcoma; operation revealed a pure lipoma. 2. A tumor was situated on the forehead between the periosteum and the aponeurosis. Diagnosis: Atheroma; lipoma was removed. 3. Cold abscess of the neck, with fluctuation, was diagnosed; operation developed a lipoma the size of a turkey egg. 4. A four-year-old girl presented a tumor extending from the spine around the esophagus and trachea to the other side. The diagnosis of struma was first made; later an abscess was diagnosed, but puncture was not successful. The patient died of asphyxia, without an attempt at operation being made. Anatomical diagnosis: Lipoma. 5. Twenty-seven-year-old butcher developed, when about 12 years of age, a crepitating tumor situated on the hypothenar eminence of the right hand; growth very slow. About three years before, the tumor began to grow faster. Examination showed a tumor half the size of an orange, heart-shapen, extending from the ball of the small finger to the middle finger. Soft, elastic, and apparently crepitating. The diagnosis of hydatids was followed by a number of unsuccessful punctures. Lipoma enucleated. 6. Patient presented a tumor in the palm of the hand, which was diagnosed as an enlarged bursa. The tumor was lobulated and movable; fluctuation was apparent, but puncture elicited no fluid. Incision revealed a lipoma. 7. A woman had for thirty years a tumor the size of an apple, situated on the second phalanx of the left ring finger. Diagnosis: Enchondroma for which the finger was exarticulated; proved, however, to be a lipoma. 8. Tumor the size of a fist, situated on the posterior inner surface of the left thigh. Had been growing for four years. Diagnosis: Myxosarcoma. At operation a subfascial lipoma was removed. 9. A 46-year-old woman presented a similar tumor to the above, which had developed within a year. Diagnosis: Sarcoma; lipoma removed at operation. 10. Lipoma the size of head, extending from the popliteal space under the gastrocnemius. Amputation of the thigh was made, consequent upon the diagnosis of cancer. 11. A 66-year-old woman presented a tumor situated between the second and third metatarsus of the right foot. The second toe was forced over the great toe, and the tumor appeared as a mass the size of the thumb at the sole. It was 5 cm. in length, and apparently fluctuating. Diagnosis: Cyst; operation revealed a lipoma.—Bertrage Z. Klin. Chirurgie, 38, 1, 1903.

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THE ELECTION OF A MAYOR.

On November 3 the public of San Francisco will be extended the privilege of casting their ballots for one of three nominees for the high office of Mayor. Among these candidates is one E. E. Schmitz, the present incumbent, who has won much notoriety because of a false and vicious policy towards the truth and the local Board of Health. On all occasions he has proven himself the enemy and contemner of public health and the medical profession. He has endeavored in every possible way to remove the present board for no other reason than doing their duty and for their failure to be overriden by an unscrupulous officer. No effort at law or political trickery was left undone and no abuse was too severe for their honesty and faithfulness in office. Fortunately for the city and State his schemes failed, but the future is full of danger. It is necessary, therefore, that every physician of this city will see that his vote is cast for the proper candidate. No one can read the Annual Report of the Board of Health for the year 1903 without fully realizing how false, infamous and wilful has been Mr. Schmitz's conduct in office. Since his assumption of office, no reports, yearly or monthly, have been published through the proper channels. All efforts to improve the health of the city have been obstructed by this same man who now appeals to the public as the champion of "Equality to All." The duty of the medical profession is very clear.

THE REED MEMORIAL FUND.

During the last few months the medical press have been urging the profession of the country to take some steps to commemorate the very worthy and great life work of Major Reed, M.D., U.S.A. The death of Dr. Reed was occasioned by overwork in his effort to make Havana inhabitable and healthy. His labor was crowned with success, and gave to the world a discovery, equal to any in medical history, that the mosquito was the greatest factor in the spread of yellow fever. This discovery has already saved thousands of lives, and the glory of this act should be commemorated in some manner, and by none more willingly than his medical brethren. At a meeting of his friends, held August 15th at Bar Harbor, the following conclusions were reached: That an effort should be made to raise a memorial fund of $25,000 or more, the income to be given to the widow and daughter of Dr. Reed, and after their decease the principal to be appropriated either to the promotion of researches in Dr. Reed's special field or to the erection of a memorial in his honor at Washington. No steps, so far, have been taken on the Pacific Coast to unite with the East in paying tribute to this great man. Surely, no physician of this State will fail in so laudable a work.

PLAGUE CASE 101.

Case 101.—Edward T. Slater, aged 31, employed by S. P. R. R. Co., at San Ramon, Contra Costa County, Cal., died in San Francisco September 14th, 1903, after an illness of a few days, of bubonic plague. The diagnosis was made before death. Bubo was situated in axilla.

SOCIETY PROCEEDINGS.

SAN FRANCISCO COUNTY MEDICAL SOCIETY.*

Regular Meeting, July, 1903.

The president, L. A Kengla, in the chair. Dr. M. E. Kibbe read a paper entitled

ASCITES DUE TO SYPHILITIC CIRRHOSIS OF THE LIVER, TREATED BY EPIPLOPEXY.

[Published at page 303.]

Dr. G. Ebright: Too conservative treatment can not be assumed in regard to these operations. The condition, even in extreme cases, can probably be relieved by medical means more often than by surgical. The operation is made under many difficulties, not the least of which is getting adhesions in a badly diseased peritoneum. Then again, it is extremely important to know that there is enough liver substance remaining to functionate. We had in the hospital a man suffering with cirrhosis of his liver with ascites, and extreme toxic symptoms which culminated in delirium and then followed coma. Apparently the man would die in a few hours. However, he recovered, had another relapse as bad as the first, and even after that got well enough to go to work. Had he been operated upon and half as good a result followed, he would have been published as a surgical victory. Only 40 per cent of improvements, and only nine patients left alive at the end of two years of all those reported, does not speak well for operation.

Dr. Dudley Tait: Referred to the common erroneous belief in the existence of only two forms of cirrhosis of the liver, the atropic and hypertrophic, both of which most physicians consider incurable. Unfortunately, the majority of text-books repeat these inaccurate views and fail to mention the great frequency of spontaneous cure in the numerous hypertrophic forms of cirrhosis. Medical societies might throw some light upon the question by exacting more accuracy and more thoroughness in reports of cases. Dr. Tait considered the communication incomplete from the standpoint of clinical observation, clinical diagnosis, and operative diagnosis. No proof had been given of the presence of a syphilitic cirrhosis, nor was any attempt made to establish a diagnosis at the time of operating. The mode of dealing with the omentum was not in accordance with the most recent methods advocated by Italian and French surgeons.

Dr. Tait's idea that the specimens were exposed to the air for an hour is certainly erroneous. The appendix, and later the gauze drain, were brought to our laboratory wrapped in many layers of sterile gauze, surrounded with gutta percha tissue. Cultures were made immediately. Fifty colonies of Friedlander's bacillus developed in the first culture tube. Certainly if we should suspect contamination it is rather remarkable that we should not have had a bacterium more ordinarily found from contamination. Also it is still more remarkable that the second culture made from the case should have shown forty colonies of Friedlander's bacillus. That Dr. Sherman's conclusions should have been "most probably entirely negative" I think is contradicted pretty strongly by the fact that the patient's blood gave an agglutination reaction with the bacillus of Friedlander in dilution of one to forty. This is certainly good evidence that the patient was suffering from an infection with Friedlander's bacillus. I think that more attention has been given to the anaerobic bacteria than Dr. Tait realizes, and I also think that the reason that this work has not been reported is because of negative results obtained. Very few pathogenic anaerobes are known. It is true that Dr. Veillon has found a number of anaerobic bacteria in such lesions as otitis media, appendicitis, etc., but it remains for him to prove conclusively that these organisms are pathogenic and that they cause these lesions, and are not simply there as contamination, so to speak, since the sites from which these bacteria have been isolated are almost all parts of the body in direct communication with the external air. It also remains for other observers to confirm his work.

As far as the bearing of Dr. Veillon's work on Dr. Sherman's case is concerned, Dr. Veillon has found his anaerobic bacteria in the gangrenous cases, and Dr. Sherman's case certainly does not belong to that group. Of the few anaerobic bacteria whose pathogenic power has already been established I do not think there is any evidence that any of them were factors in the case. In conclusion I should like to say that the methods pursued in this case are those pursued in the vast majority of similar cases both here and abroad; and it remains for some one to prove that other methods are necessary before such methods will be adopted.

Dr. Kibbe: I wish to thank the gentleman who champions the cause of the more recent graduate. Replying to the gentleman who would be slow to advise operation in these cases, because of the severe atrophy of the liver usually present, I will say that, in this case, the liver was enormous, and that the result of the operation is its justification. With regard to the last gentleman who spoke: As he was not present when most of the paper was read, I may be excused from answering him.

SOME SURGICAL USES OF PARAFFIN

Dr. Morton presented the following patients: Case I. Removal of superior maxilla for carcinoma; later the deformity was corrected by injecting at two sittings 40 c. c. of paraffin. Case 2, only one of this class. Has suffered past four years from necrosis of tibia, due to injury; the necrosed bone was chiseled out and the space filled with paraffin, and the periosteum stitched over, with good result. Case 3. Depression of nasal bridge due to syphilis. The bridge was elevated with forceps and retained by two injections one month apart. Case 4. Lateral deformity of nasal bridge from injury; crushed, straightened, and imbedded in paraffin which hardened and retained the fragments. Case 5. Restoration of congenital deformity of nasal bridge, by injection of 8 c. c. of paraffin; no bad effects. Case 6. Atrophy of testicle—injected 40 c. c. of paraffin with good results. I have been using the ordinary paraffin, having it at a melting point of 109° F. by diluting it with sperm oil. It is then sterilized by simply placing the paraffin in a water bath, and boiling. The syringe I use is a Lure's syringe, as it has two distinct advantages over other syringes; it is always in working order, and secondly, you can see the condition of the paraffin at the time you make the injection. The needle is a large size hypodermic, and is readily attached to the syringe by a joint. The first case is a gentleman who has been suffering with carcinoma for about two years, of the right superior maxillary bone. He presented himself at the City and County Clinic last December, where we diagnosed the carcinoma of the right superior maxillary. We injected him with cocain and proceeded to remove the mass, and the entire right superior maxillary bone back to the zygoma. He recovered in a very short time, but with the characteristic deformity, where you have the absence of a superior maxillary. We, at two different sittings, injected into this space just beneath the skin 40 c. c. of paraffin with the melting point 109°F., and built it up so that it bridged over. There was a slight swelling which lasted about a week. The man was discharged, but returned one month ago with carcinoma beginning to fill in the right nostril. We dissected out both external carotids, cutting off the blood supply to see the result. The next man is the only one that I have attempted to use paraffin on in bone surgery. He has suffered for four years with necrosis of the bone as a result of an injury, and some osteomyelitis. We operated on him three months ago making an incision, chiseling out the inner surface of the tibia for about six inches. We were able to lift the periosteum and fill in an enormous quantity of paraffin, then stitched over periosteum, and we have had a beautiful result. Just what factor the paraffin has been taking I am unable to say. Paraffin acts somewhat as a blood clot for bone tissue to fill in. We have here another class of cases, where we have the entire absence of the bridge of the nose due to syphilis. We injected paraffin in from each side, at the same time holding the bridge upward to prevent interfering with breathing space. First injection caused irritation; we waited one month and then made a second injection. The second case was one where we have a lateral deviation of the nostril, with a slight depression in the center line. In this case the patient was placed under a general anesthetic, the septum crushed and the upper portion of the superior maxillary bone fractured, then pressing the nose to the opposite side. The nose does not have tendency to remain in that position, but I have found by imbedding the paraffin it seems to act as a splint, and succeeds in holding it in good condition.

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*Owing to a serious error in the "make-up" of these proceedings, they are republished in order that the transactions of our Societies may be kept in as complete a form as possible.

Dr. A. B. Grosse: I have been very much interested in Dr. Morton's case, but not having sufficient experience in much of this work I will confine my remarks to cases pertaining to my specialty. If Dr. Morton has succeeded in repairing the deformity of syphilitic saddle nose as he describes, i. e., injecting paraffin from both sides of the nose and in that way building up the bridge, he certainly accomplishes surgical impossibilities. For when the bridge and nasal bones are destroyed by syphilitic disease the soft tissues sink down and are tied down by dense adhesions. In my cases and in all other cases published up to date, the adhesions had to be carefully severed by a tenotomy knife or some other specially constructed instrument and the paraffin injected from either side, moulding the shape of the nose to suit the wishes of patient and operator. The description of Dr. Morton's technique seems delightfully simple, but with this very method I have had considerable difficulty and trouble. I have used the same type of syringe and needle but found that the paraffin always congealed before I could inject into the tissues. After repeated trials and by heating needle and syringe I practically overcame these difficulties. In April, 1903, I read the reports of H. Eckstein Berlin who has constructed a syringe which can be heated without damage and must for that reason be considered a distinct advantage. Also he uses his paraffin of high melting point, as it sets more readily and is not absorbed but encapsulated.

Dr. H. M. Sherman: I am also interested in this subject. I had a case ready for this procedure when I heard Dr. Morton deal with this same subject in Santa Barbara. I have had the same difficulty of which Dr. Grosse spoke. The paraffin congealed in the needle. This was a case of broken nose, and I found it impossible to get the paraffin through that part of the needle which was outside of the skin without solidifying.

Dr. Stapler: I have had some experience with paraffin injections. I made several injections in animals and human beings. I injected large quantities in rabbits. The paraffin will spread in rabbits all over the body, owing to the looseness of their skin. We injected it also into the abdominal cavity, where it formed large plates which were adherent to the omentum or intestines by dense fibrous exudations. If we inject paraffin under the skin, it will always spread where the connective tissue is loose. We find after some weeks or months in the surrounding part new connective tissue. In some cases we will find giant cells, mostly where hard paraffin was used. After a few months the paraffin is divided into very small particles by the invading of connective tissue, and we will see in the microscope that the tissue looks like a sponge, and inside are small particles of paraffin. The cells surrounding the paraffin are flattened and look very much like endothelium. I mixed chlorophyll with paraffin and injected it into rabbits; the lymphatic system of rabbits differed from the human. Absorption takes place very much quicker. After a few days we find the lymphatics invaded with chlorophyll, even on the other side. We will always find also in human beings paraffin spreading toward the loose connective tissue. In December, 1900, I injected in my finger one c. c. of paraffin with a melting point between 56 and 60°, and if you will examine my finger you will find that the little lumps spread toward the center at the same proportion the lump gets smaller. We will find beside the absorption of the paraffin a displacement in the tissue. Dr. Morton said he injected 40 c. c. in one man. I find that 8 to 10 c. c. is the highest we should inject at once, if we do not want to have an intense reaction, such as chill and high fever beside redness and swelling. These injections are not entirely devoid of danger. Many cases of emboli in the lungs are reported. In one case a small quantity injected in the nose was promptly followed by blindness on one side. I prefer paraffin of a low melting point, 39° to 42°, because it gets just as hard as hard paraffin after a few weeks and the danger of causing emboli is very much smaller. Paraffin of a low melting point facilitates the technique enormously.

Dr. Rosenstirn: I had occasion to also form a testicle in a case where on account of bilateral tubercular epididymitis and orchitis I had to remove both epididymis vasa deferentia and testicles with the exception of a small fragment of one which I could leave. The patient developed a very severe melancholia and threatened to commit suicide. With Dr. Stapler's assistance we injected on the left side, unknown to him, a certain amount of paraffin, and in that way formed a testicle which seemed to have the reassuring desired effect upon him. I would also like to speak about the patient of Dr. Morton's with the bone cavity in the tibia. All over the part that had been exposed by chiseling the skin is adherent to the bone. There is an ulceration of the skin and a suppuration and partial elimination of the filling going on. My plaster of Paris method shows better results. The cases which I have had the good fortune to have under my care have healed with movable skin over the filled bone. That is the skin is not adherent and inflamed as in this case, but has a perfectly normal appearance. I may add in this discussion that one of my patients, a man who suffered from general osteo tuberculosis, died from tuberculosis of the pelvic bones two or three years after a filling of the entire right radius had completely healed. I took this radius out and found it quite restored, no recurrence, and one could still detect in the new formed rather sclerotic spongiosa some parts of the plaster of Paris with which the entire cavity had been filled, but only in small specks, the balance had been absorbed and replaced by new bone.

PARAFFIN IN NASAL DEFORMITIES

Dr. Morton: The next case is one of those congenital characteristic saddle back noses. The operations for restoring this deformity have been very difficult, until the use of paraffin. The injection of 8 c. c. completely restored the deformity without any inflammatory reaction. The next case is one of unusual interest; at the age of three years he had an injury to his left testicle; the testicle had completely atrophied when he called my attention to it some six months ago, wanting the absent member restored. We injected into the scrotum about 40 c. c. of paraffin, moulded it into position, and to-day the testicle has all the appearances of a normal one. In a number of cases I have found adhesions of the skin, generally in syphilitic cases or from traumatism. In such cases I have found two ways of separating the strong adhesions. 1st, by infiltrating with water; 2d, with a very delicate scapel, which I have slipped just beneath the skin, separating all the adhesions, then infiltrating it with paraffin. The greatest objections have been that the needle became obstructed. We overcome that objection by first introducing the needle and attaching the syringe when ready for use, which can be readily done as it is attached by a joint. The melting point of the paraffin is a very important feature; if used at a high melting point it is more difficult to introduce, as it is liable to coagulate, and then there is danger of necrosis, or sloughing. If it is used near the temperature of the body it may not coagulate soon enough to support the parts, and there is more danger of embolisms. I have found the melting point of 109° F. very satisfactory in thirty-six cases. The change which takes place in the paraffin when deposited in the tissues is very interesting, and has caused much discussion. In one of my early cases some of the paraffin pressed below the turbinates. I removed a section of this each month for five months, and found at the end of that time that the entire mass was converted into connective tissue. It would appear that the paraffin acts simply as a blood clot for the time of repair, and the relief of the deformity is permanent. To prevent the paraffin from slipping into the sides, it is well to keep firm pressure with the fingers at the time of the injection. It has been used in hernia to plug the canal, but I can hardly imagine a condition where it would be better than the radical operation. Another gentleman spoke of the danger of emboli; this danger can be easily overcome by using the paraffin with a melting point above the body heat, so that it will coagulate when deposited in the tissues. The experiments of Dr. Comstock, of Minnesota, are conclusive; he injected animals with the melting point of the paraffin below the body temperature, all of which died in two weeks from emboli.

Dr. H. M. Sherman presented a

REPORT ON TWO OF THE LORENZ CASES

Dr. Sherman: In each case the original dressing had been taken off and the child immediately radiographed. Examination of each then showed that the femoral head was in front of the trochanter, the latter was close to the acetabulum but to its outer side. The former was in a subfacial location in the outer part of Scarpa's triangle. It was difficult to see what had been gained by the operation. One of these cases was submitted to an arthrotomy. By this it was found that the manipulation operation had torn the femoral capsule, and thrust the head through the rent out among the muscles. In this position a thin adventitious capsule had formed about it, but the femoral articular cartilage had become eroded and absorbed. The proximal part of the torn capsule was adherent to the acetabular floor and was a positive obstacle to reduction of the dislocation. After it had been removed the head was put in the acetabulum, but this cavity was very shallow. The outer case was not cut down on and it was assumed that the conditions here were like those of the first case. The boy has been permitted to walk about. The head is still in an anterior location, but the posterior one is a possibility. Dr. Sherman promises further reports of these cases.

Dr. Stapler: I intended to bring you today the first patient of Dr. Lorenz, a little girl of eleven. Unfortunately the patient has left the city. I wanted to show you this patient; not because she has the plaster of Paris, not because she was operated upon by the bloodless method, but I wanted to show that the treatment of the congenital hip does not lie only in the manipulation in the application of a splint, but mostly in the after treatment. This was neglected in several cases. First of all Lorenz insists that they must walk. That is very important because we know that there is no acetabulum, no normal head, and all we can do by the bloodless operation is not to make a new head, but to make way for a new joint. This child will use its limbs more or less normally. If the child walks it presses the abnormal head in the abnormal acetabulum and forms a new point. It is very important to let the child walk and run. There is another child I know on which he operated, which never walks but sits in bed. I think we should not judge any of his cases just right after taking off the splint, but after manipulating and treating the patient from one to two years as Lorenz does. Whether the head is in the acetabulum or not, higher or lower, as long as it is of good use for the patient, the functional result only counts. It is like in a old luxation of the humerus; there it is far better to make a bloodless reduction than to make a resection of the head in order to have a joint that will do the work. We should judge these patients after one or two years to see how much we have accomplished.

Dr. Terry: I want to ask if Dr. Sherman thinks it would be unwise to attempt the Lorenz operation in these cases—if there is anything against doing it first, and then if it is a failure, doing something else. I did one of these operations and was pleased with the result. The reposition is apparently perfect, and the X-ray shows the head of the femur in the acetabulum. In that case the child was younger and the acetabulum deep enough so that the head of the bone stays in place.

Dr. Kenyon: Dr. Sherman's paper accentuates one of the difficulties in this class of cases. I refer to the capsule, the ligamentous tissue filling into the already imperfect acetabulum and filling it up. When I began the treatment of the fractures of the patella, I practiced entirely the fixed dressing and I found great difficulty in approximating the parts, I did not then fully appreciate the difficulty in the effort of approximation until I began to operate in those cases. Those who have operated on cases of fractures of the patella will note that the periosteum has a tendency to fall in between the fragments preventing osseous union. In the case of operation, the periosteum is lifted up and the osseous parts are brought together. This ligamentous tissue of the capsule of the hip, which is not very absorbable, finding a resting place in the acetabulum permanently keeping the head of the femur from its normal position, prevents a favorable result in the restoration of a congenitally dislocated hip joint.

Dr. Sherman: I do not know but what Dr. Stapler is right in a certain way. The cases have not been in existence, as operated upon cases, long enough. There are certain things which have come up which have surprised me. The effort of Lorenz, as I saw him work, was to put the head in the acetabulum, and he put on the splints to keep such reduction as he had succeeded in getting. The after treatment, of course, is of moment, but not of the only importance. I have seen some of these cases which were not my own patients. One in which the child had not walked, and in that case the femoral heads were still on the acetabulum but not in them. The children who had walked had pushed their femoral heads away from the acetabula, some to the outer part, some to the inner part, but the head was decidedly away from the acetabulum. Walking had produced a new dislocation. It had not in this instance made an acetabulum. The capsule question is the real question in the whole proposition. If you have a capsule elongated, but not constricted, and can pass the head down that capsule into the acetabulum, and you put the leg up in a proper position, you can keep it there and can develop an acetabulum. That hip may be exceedingly useful. In such a case of mine the child afterwards developed a vertebral tuberculosis and later a tuberculosis of the opposite hip. He has gone through the treatment appropriate for these conditions and now is walking on his congen dislocation hip (the reduction being permanent) and crutches, while the spine and other hip are in braces still. Oshner, Jr., of Chicago, made a reduction of this character. He kept the case carefully under his supervision as long as possible, and the people promised to let him know if anything ever happened to the child. He received notice of the child's death and he has the specimen—normal on one side and the reduced dislocation on the other, and you can not tell these apart except for the absence of the ligamentum teres. It is only where the capsule happens to be very lax that Dr. Lorenz can with any possibility produce an anatomical reduction. If we could be sure that we could make the reduction, yes, then we should use this method; but if one's efforts were carried to such a point that you lacerate the capsule and thrust the head out among the muscles, then *no.*

Dr. Sherman reported a case of

ACUTE SUPPURATION APPENDICITIS DUE TO BACILLUS OF FRIEDLANDER.

The case began in the usual way, and was submitted to operation within the first twenty-four hours. The belly was full of sero-pus and the appendix perforated. The usual removal of the appendix was practiced, the belly was drained with gauze and the case made an uninterrupted recovery. The investigation of the appendix and of the first drain after its removal gave pure cultures of Friedlander's pneumo-bacillus. Dr. Mary Halton had done the bacterioscopic work and had proven by getting clumping of the bacillus with the patient's blood that the organism had been an infecting element in the case. Dr. Sherman commented on the fact that he had not seen this pathogen credited with this lesion before.

Dr. Dudley Tait in discussing Dr. Sherman's case: I wish to compliment my distinguished friend, Dr. Sherman, on the character of the work done at the Children's Hospital, from the various services of which we expect to receive from time to time some very interesting facts. Many of our hospitals, on the contrary, never even think of utilizing their cast material, while others furnish yearly long statistics of operative work alone, which closely resembles and almost deserves the name of "surgical sport." Dr. Sherman's findings are certainly interesting, and I beg to remind him that with the aid of modern laboratory methods his conclusions might have been, or rather most probably would have been, entirely negatived. To expose a specimen for more than an hour to the air and then take a culture from it, and furthermore to make culture from a gauze drain twenty-four hours after operation may be considered sufficient to suggest probable contamination. Many suppose that a few culture agar or bouillon tubes, an incubator or a convenient hip pocket are all the requisites for a bacteriologic diagnosis. They firmly believe that such a diagnosis is as easy as a qualitative analysis in inorganic chemistry. Unfortunately, many of the text-books hold a similar opinion. They forget that a very important class of pathogenic bacteria will not grow on the ordinary media in contact with air. If, for example, we take a specimen of pus from an acute otitis or a marked appendicitis and examine immediately with or without stain, we shall notice an extreme variety in the morphologic aspect of the specimen. Upon culture of this pus on the ordinarv media, we shall be surprised at the enormous disproportion between the abundance of micro-organisms found upon direct examination of the pus and the small number of colonies developed. The conclusion is obvious; the majority of the bacteria have not developed. In some cases, the culture tubes remain absolutely sterile and then the hasty or ignorant observer concludes to the sterility of the pus. In case some rare colonies do appear they generally belong to the common pyogenic varieties. The majority of the forms noticed in the pus are not present. Now, experience shows that if instead of using the ordinary media as for aerobic bacteria, we make cultures on media deprived of air, an abundant growth of anaerobic bacteria takes place and one finds again the numerous forms noted originally in the pus. I have never understood the silence of the English and the American school upon the subject of the role of the anaerobic bacteria. Very recently in the Hunterian lecture upon the pathology of appendicitis not one reference was made to this all-important subject, which the French school has studied most carefully ever since Pasteur, in 1861, first isolated and cultivated the first anaerobic micro-organism. In 1889, while working in the Pasteur Institute at Paris, I saw considerable attention given to this class of bacteria, and during the past five years excellent results have been obtained, especially in the study of otitis, gangrene of the lung, urinary infections, and anglo-cholitis and appendicitis. In a series of twenty-two cases of appendicitis reported in the "French Archives of Experimental Medicine" in the latter part of 1898, a most careful bacteriologic search showed the presence of anaerobic bacteria in every case but one, this being a mild pneumococcic infection. In many cases the lesions were solely due to the anaerobes. The most practical method of studying anærobes is with the Liborius tube, as modified by Veillon, which permits of an easy segregation of the aerobic, facultative and obligatory anaerobic bacteria. In conclusion, I would suggest that we resort to the more accurate methods of bacteriologic diagnosis, even if such methods be delicate and require several weeks to investigate a single case.

Dr. Sherman: I do not think that I ever derive more profit than I do from the criticism of Dr. Tait, and I am very glad that he went over this question for me. The possibility for contamination, of course, is acknowledged. If it had not been that the cultures came out as they did, this paper would not have been read to-night. As Dr. Halton says, we may be perfectly certain that the pneumobacillus was in that patient. It would be impossible for me, or for any one to carry a sterile material from one place to another and infect it with the same organism. Concerning the methods of diagnosis with regard to the use of tubes, etc., the criticism is good and I agree with it. I learned to carry sterile tubes in my pocket from Dr. Tait, and I find them to grow very well indeed when inoculated. The question of the bacterioscopic work I have left to Dr. Halton. The change which came over my idea regarding the case, after what I had supposed to be a fatal condition, made a great impression upon me, and therefore I had it followed out. Looking for organisms which are in the appendix is simply a routine matter. I should be inclined to consider that Dr. Halton's opinion on the presence of these organisms in this patient would have to stand.

Regular Meeting, August, 1903.

The president, L. A. Kengla, in the chair.
Dr. A. B. Grosse read a paper entitled

TREATMENT OF LUPUS ERYTHEMATOSUS AND REPORT OF A CASE

Dr. R. D. Cohn read a report on

ACUTE RADIATING PARENCHYMATOUS KERATITIS, DEMONSTRATING A CASE

[Published at page 375.]

Dr. V. H. Hulen: There is not much to add to what Dr. Cohn has told us concerning this case. Our thanks are due him for the privilege of examining this patient, for the case is most interesting and a condition not often shown to a society in a patient of her station in life. I am inclined to think that the disease is the regular parenchymatous keratitis, and not the variety called "keratitis profunda" by Fuchs in his book. The appearance of the corneal lesion seen here is quite peculiar, but not significant in my opinion of any rare corneal disease. Its duration in the first eye was unusually short, it is true, for interstitial keratitis, but the prompt and vigorous correct treatment applied may account for that. From its rapid response to antisyphilitic treatment, and also from its occurrence in the other eye some weeks later and from the appearance of the patient, I am inclined to think the cause specific, but of this we can not be sure one way or the other. The sudden appearance of the disease, as described by the patient, is most likely a mistake on her part. It probably began in the first eye about as it did in the second, but the patient discovered the disease in its incipiency in the latter case because she was on the lookout for it. Oculists not infrequently see people practically blind in one eye who did not discover the condition until years after its occurrence.

Dr. Nagel: I agree that this is a case of parenchymatous keratitis. This disease of the eye is not due always to lues; in twenty-five per cent it is due to tuberculosis. I should like to ask whether Dr. Cohn has examined the fundus of the first eye. We find in a large percentage of cases peripheral chorioiditis after the keratitis has passed and we are able to look into the eye. I should like to know whether he has examined the eye with the loup mirror. In most cases of parenchymatous keratitis we find permanent bloodvessels in the cornea which are not visible otherwise.

Regular Meeting, September 8, 1903.

The president, Dr. L. A. Kengla, in the chair.

Dr. T. Huntington exhibited a case where he had done a

TRANSPOSITION OF THE FIBULA.

Dr. W. I. Terry: This case has interested me exceedingly, and I think that it is worthy of considerable interest. The union of the upper end of the tibia with the fibula is somewhat against the experience of some who have done this work before. I have heard of some cases where the fibula was switched into the tibia in this manner and has gone back to unite with the end of the fibula, due apparently to the powerful muscles which pull it back. In this case there seems to be no tendency for that to happen. There were no silver wire sutures employed in this case.

Dr. M. Krotoszyner read a paper on

OBSERVATIONS ON THE PROSTATE GLAND IN ITS RELATION TO GONORRHEA.

He said that in the beginning of his studies on the relation of gonorrhea to the prostate, about ten years ago, text-books dealt with two pathological processes of the prostate as sequela of gonorrhea,—the prostatic abscess, and spontaneous appearance of prostatic juice at the meatus, or prostatorrhea. Prostatitis as a complication of chronic gonorrheal urethritis was then unknown. The onset of gonorrheal prostatitis can not, in the majority of instances, be ascertained, since no characteristic symptom points to the invasion of the prostate by gonorrheal infection. The diagnosis of gonorrheal prostatitis can only be made by palpation of the prostate and macroscopic and microscopic examination of prostatic fluid. In the experience of the writer, in nearly fifty per cent of chronic gonorrheal orchitis, an involvement of the prostate was diagnosed. The writer dwells upon the characteristic palpatory evidence, and also upon the most important points of macroscopic and microscopic examinations of the prostatic fluid. The normal gland secretes a milk-white fluid, while the diseased one furnishes a fluid turbid and floccular in aspect. The presence of nests of round cells is particularly noteworthy. Since in the healthy gland fat is always found in abundance, its absence or decrease must be interpreted as a pathological phenomenon. Gonococci can only be found for about the first six months after the onset of the infection. Whenever the glandular tissue of the prostate is once invaded by infectious material, a successful treatment is tedious and a cure a matter of grave doubt. In time the gonococci will disappear, but the other microscopic findings of pathologic note remain stationary. Therefore the writer did not object to matrimony in his patients whenever after repeated examinations gonococci were missing, even if his slides showed round cells. Great stress is laid upon general neurasthenia as a sequel of gonorrheal prostatitis. As regards classification the writer differentiates clinically between a total parenchymatous and a partial follicular form, between a prostatitis with and one without a coexisting urethritis and finally between a latent, chronic prostatitis with subjective symptoms and that form of prostatitis observed in sexual neurasthenics. The treatment of goncrrheal prostatitis must be based upon an exact diagnosis. Active therapy to the prostate should be delayed until acute and inflammatory conditions have abated. Massage of the prostate is the most important therapeutic agent if done carefully, at the right time, and at correct intervals. Massage is followed by irrigation with silver solutions of various strengths. Whenever a posterior urethritis is present, massage is followed by dilation of the urethra, either by steel sounds, or by Kollman's dilators. In acute and subacute forms of prostatitis local applications of hot water, through Artzberger's instruments, preferably the double-channeled, permitting the application of hot and cold water, and its repeated change at the same sitting is advocated. Of drugs, ichthyol and iodide of potash in form of suppositories or rectal clysmas are recommended. Mild faradization of two to five minutes' duration is valuable. Most important is the general roborative and psychical treatment of neurasthenia after gonorrheal prostatis.

Dr. A. B. Grosse: I congratulate the doctor upon the excellence of his paper and upon the fact that he has called the attention of this society to the importance of involvement of the prostate gland in gonorrhea. For some time I have been surprised at the number of cases with chronic urethral and prostatic involvement who consult me, after having been discharged by others as cured. In the main, I thoroughly agree with the doctor; the slight differences of opinion I will state briefly, and would like to ask for further information upon some points. I would like to ask Dr. Krotosyzner if he considers a case as cured, with pus cells still present in the prostatic secretion. Of course, it is a well-known fact that simple catarrh of the prostate is a benign but an almost incurable condition, being particularly stubborn if accompanied by phosphaturia. It has been proven and accepted for some time that the prostate is a very poor medium for gonococci, and that the same can not be demonstrated in the secretion of this gland, after an involvement of six months or more. Still, if the prostate be massaged, or an irritation set up in any other manner, gonococci can and have been demonstrated culturally. It seems to me that the doctor misquotes Professor Finger, with whose methods I am fairly familar. Professor Finger does not forbid marriage on account of the presence of simple catarrhal involvement of prostate; to the contrary, even with the presence of pus cells, after a large number of microscopical, as well as cultural, examinations, with negative results as to finding of gonococci, he permits marriage. The obtaining of fairly pure prostatic material, in a case of coexisting urethritis, at least for clinical purposes, is not very difficult; irrigating anterior urethra, having patient urinate, and then massaging prostate. This method, with slight varition, is most frequently satisfactory. A treatment that I have found of some use recently, particularly for peri-glandular infiltrations, is vibratory message of prostate. Finger advises massage of prostate gland, because secretion is expressed from small ducts, and so, when deep instillation of $AgNO_3$ is made, some of the silver salt reaches the gland through the medium of capillary attraction. To my mind the beneficial effect of message is due to stimulation of circulation in the glandular substance, as well as to the evacuation of irritating, i. e., purulent secretions, affecting this condition in the same manner as drainage an infected wound.

Dr. R. L. Rigdon: The author of this paper is to be congratulated upon having presented this subject to us in a manner at once plain and convincing. The class of cases to which the doctor refers furnishes some of the most difficult therapeutic problems with which we have to deal, and all investigation or observations that make more definite or precise our knowledge of the morbid anatomy of the parts involved will point the way to a m scientific and successful therapeusis. There are two points to which I wish to call attention. First: It is often difficult to determine upon clinical examination the extent of the prostatic involvement. The examining finger is swept over the prostate and finds the normal limits obscured. There is a gradual shelving off of the infiltration into the adjacent tissues. This is especially apt to be the case along the trigone of the bladder, for here the prostatic follicles seem to extend beyond the prostate proper and to reach up into the bladder wall. Second: In a certain proportion of cases the circular fibers about the vesical neck take on a condition of fibroid contraction, resulting in a more or less decided obstruction to urination and giving rise to various other symptoms. When this contracture is present it must be recognized, else all our efforts to effect a cure will be in vain.

Dr. Rosenstirn: I have only one thing to say in the discussion of this very able paper, regarding Dr. Krotosyzner's confidence that a few examinations of the secretion of the prostate continued for a short time should, if they fail to show any gonococci, guarantee against the reappearance without a new infection. I believe the gonococci lodge for a long time in the lacunae of the urethra, much more so is the prostate able to retain foci of gonococci which upon one occasion or other, perhaps after a bacchant or venereal debauch, a merely traumatic irritation will send forth new immigrants to the external orifice of the urethra, and we will meet them again a month or so after we have ascertained the absolute freedom from gonococci, and our former verdict has to be revised. I certainly believe that Dr. Krotoszyner has observed cases which, after he ascertained them to be free from urethral and prostatic secretions, will again show gonococci.

Dr. W. Winterberg: With reference to the question, Shall patients with chronic gonorrheal prostatis be allowed to get married? I wish to mention some cases that came to my personal knowledge, in which marriage was contracted while the disease still continued, and still no evil consequences have resulted. The first case was one of a man who got married about twelve years ago, while having a discharge from the urethra. When I saw him first, he was the father of six healthy children; his wife was likewise healthy, and never had been sick during her married life; a fluid containing pus cells was obtained by expression of the prostate. The second case was that of a man who married when still suffering from chronic prostatitis, and never transmitted the disease to his wife. There were two children born to him, the first being healthy, the second one being an idiot without speech and power of locomotion. I can not see any connection between the condition of this child and the father. In this case also the juice obtained by expression of the prostate contained pus cells. In a third case, with analogous conditions as to the health of the man, two healthy children were born, one of whom I had to treat for gonorrheal ophthalmia for a couple of weeks after birth. The mother did not suffer from any complications. I am far from encouraging marriage of men who are afflicted with chronic prostatitis; at the same time, I think that the danger of infection in cases where no gonococci are found is rather remote. The reason is, possibly, that, while gonococci have a motion of their own, pus cells have not, and that therefore the latter are less apt to invade the uterine cavity and Fallopian tubes and produce the usual symptoms of such invasion.

Dr. M. Krotoszyner: In answer to Dr. Grosse's question whether in my opinion chronic prostatitis is curable, I will repeat what I have said in my paper, that if the glandular tissue of the organ is once invaded by gonorrhoic infection, a cure seems to be impossible. The microscopic features of the infection, especially the presence of nests of round cells, can not be influenced by any treatment. But experience has taught us that a prostate that under repeated examinations does not reveal juice containing gonococci, even in spite of the presence of numerous round cells, is rather harmless as regards infection to others, and void of any discomfort or dangers to the patient himself. I would be guarded in the application of vibratory massage to the prostate; no instrument possesses the fine touch that our finger does, and may, therefore, in many cases do more harm than good. I beg to differ from Dr. Rosenstirn's opinion as regards to the presence of gonococci in the prostate after a long period. The prostate is not a medium in which gonococci will flourish a long time they generally assume after some time degenerative changes, and are difficult of recognition as such. Rarely only unmistakable gonococci are found in the prostate a year after the infection. Therefore I do not think Dr. Winterberg's case was due to gonorrhoic infection. The cases of which Dr. Rigdon spoke may certainly have been due to chronic gonorrhoic prostatitis involving and narrowing the neck of the bladder. In such instances good results may be obtained by massage of the gland and rational local treatment. In advanced cases, of course, Dr. Rigdon's operative procedure, as reported on another occasion, has to be resorted to.

Dr. H. D'Arcy Power read a paper entitled

RODENT ULCER, WITH REPORT OF A CASE.

Dr. Barbat: While rodent ulcer is comparatively rare, we run across these cases occasionally. I remember one which I had some years ago. The doctor spoke of one point: that is the fact that a patient who has rodent ulcer will last longer than one with epithelioma. Rodent ulcer does not produce the same amount of infection in the system. The one I saw involved the entire portion of the frontal bone, orbit, nose, and both superior maxilla. The cerebral cavity had not been opened; brain not protruding; dura mater prominent. In that case the body was well nourished, and kept so until the last.

Dr. D. A. Stapler read a paper on

WHAT CAUSES APPENDICITIS?

Dr. T. W. Huntington: Dr. Stapler's paper is commendable, as showing work done along a line which is of extreme importance. In spite of all that has been brought out upon the treatment and general management of appendicitis, the last word regarding the etiology of the disease has not been spoken or written. The important principle underlying this subject has been thoroughly dealt with by the reader, who, in my opinion, has struck the keynote to the situation. The term "infection" is the word upon which the etiology in this relation hinges, the word to which we must always look as expressive of the cause in every case of appendicitis which is manifest in physical signs. I do not propose to discuss this question from my own standpoint or from my own experience. The laboratory worker has taught us the all-important lesson. From that source we have evidence which is altogether conclusive and satisfactory, with which possibly we are not all familiar. I refer to the very elaborate work of Frazier, of Philadelphia, which was published in the Pepper Memorial Volume, in 1900. A review of his investigations which were prosecuted upon rabbits will not be out of place in this connection, and I shall take the liberty of systematically quoting from him at considerable length. His experiments were instituted to determine, if possible: 1. The effect of aseptic ligation of the appendix, the ligature being applied so as to close the lumen without interference with the integrity of the structure or the nutrient vessels. 2. Effect of similar ligation in presence of a foreign body in lumen. 3. Effect of a foreign body in lumen without other interference. 4. Effect of closure of lumen upon virulence of infection. 5. Effect of virulent infection in the lumen without other interference. 6. Effect of infection added to chemic irritation without other interference. 7. Effect of infection plus a foreign body without other interference. 8. Effect of ligation of nutrient vessels individually and together.

The following results were noted after long and repeated experimentation: 1. Closure of lumen verified the findings of Roger, Josue, Talomon, and Dieulafoie. In every instance suppurative appendicitis ensued. 2. Closure of appendix plus a foreign body in the lumen gave results identical with the preceding. 3. A foreign body in the closed lumen seemed to exert no influence upon the appendix wall. 4. The effect of closure of lumen upon the virulence of infective germs normally contained in the appendix gave findings of special interest. These are shown in the following experiments: A rabbit's appendix was exposed and ligated at base, and a culture from lumen was made upon bouillon. Twenty-four hours after, a guniea-pig was inoculated with two c. c. of first culture. On the seventh day the wound was reopened, and a second culture was taken from the now distended appendix, and twenty-four hours later five guinea-pigs were inoculated with the second culture. The results were as follows: Culture No. 1, taken during first operation, produced only slight local disturbance, and the pig recovered promptly. The five guinea-pigs inoculated with culture No. 2 all died at periods varying from thirty hours to eight days. These studies proved conclusively that obstruction of the lumen for a brief period is capable of converting an inoffensive into a most virulent organism. The post-mortem findings also proved that the wall of the appendix underwent marked pathologic changes. Gangrene was present in a fair proportion of cases, although the nutrient vessels were unmolested. 5. Introduction of a culture taken on the seventh day into the unobstructed lumen gave a negative result. 6. A virulent culture in the unobstructed lumen in the presence of chemic irritation gave a negative result. 7. A negative result was also obtained with a virulent culture in the presence of a foreign body. 8. Ligation of the vein and artery simultaneously produced marked structural results, of the artery alone less marked, and of the vein alone the effect was unappreciable.

From the foregoing it is apparent that two factors play an important part in the causation of pathological changes in the appendix. Obstruction of the lumen seems first in order, because of its bearing upon intensity or exaltation of infectivity, with all the possibilities suggested thereby. The second factor rests upon circulatory disturbances, especially where the vein and artery are both obliterated. This is due undoubtedly to the production of lessened resistance, whereby an infection which otherwise would be innocous obtains a permanent foothold, and with resultant destructive changes. The insignificance of the presence of foreign bodies seems to have been clearly demonstrated. There can be do doubt that a foreign body of sufficient size or in such location as to cut a figure in obstruction may become in that relation a possible menace; but when it lies loosely in a perfectly-drained appendix, it may be tolerated for an indefinite period.

Dr. Rosenstirn: The remarks of Dr. Huntington were extremely interesting, but I think they do not exactly cover the ground of Dr Stapler's paper. The keynote of his argument was to point out the cause for the infection. Appendicitis undoubtedly is brought on by infection, but where and how is the entrance portal formed? Colon bacilli are most plentiful all throughout the intestinal tract, but require a lesion in the protective epithelial tissue to display their zymotic action to such a lesion due to a foreign body causing a pressure necrosis in the wall, or to capillary hemorrhage in the wall caused by exertion. Dr. Stapler advances the idea that traumatism causes capillary hemorrhage within the wall of the appendix, leading to tissue necrosis, whereby the entrance of the infection is made possible; where, in other words, bacilli can enter. I grant it may be so, but I would believe it more readily if a series of microscopic specimens would demonstrate it to me. We ought to see that such capillary hemorrhages within the wall occur in cases of appendicitis, and that they cause necrotic erosions or ulcerations of the mucosa. If that is shown, it will give us a better understanding of the modus operandi of the infection in its first stage. These interesting experiments of Francis, quoted at length by Dr. Huntington, point in a way to Dr. Stapler's theory because ligation of the appendix will certainly produce a capillary hemorrhage within its walls, and a necrosis might follow. The entrance for the infection may be thus opened. At the same time the demonstration that the foreign body in the appendix alone without ligation will not produce a pressure necrosis, also speaks for Dr. Stapler, and the results of ulceration, subsequent strictures, subsequent partial occlusion, may be due to primary hemorrhages that have caused ulcerations, which in their turn again have caused strictures from cicatricial contraction, or occlusions by opposite ulcerated walls growing together. Experiments in that direction with serial microscopical sections of fresh and older appendix cases may tell the tale in the future.

Dr. D'Arcy Power: We know well that constipation is constantly attended with appendicitis. That has been put forward as one of the direct causes. That will have some little bearing upon capillary hemorrhage. Constipation increases the blood pressure when straining at stool. The only thing I can say against this is that, whilst constipation is a great deal more common among women than among men, appendicitis is the opposite.

Dr. Kugeler: I should like to ask whether in America we use the term "Jacob's Ulcer" for any other condition than rodent ulcer. While visiting Sir Wm. MacEwen's clinic in Glasgow, he asked me this question before his class one day and I told him, "Not that I know of, in fact the term 'Jacob's Ulcer' is only imployed by a few of the old surgeons." With all due deference to my friend, Dr. Stapler, I must say that I remember reading a paper very similar to his in the journal of the A. M. A. about a year and a half ago. This may be another case of the so-called mental telegraphy we have heard so much about of late. This was before the doctor came to this city, and, of course, the A. M. A. and its journals are rather unknown in Europe. In this paper stress was laid on the occurrence of appendicitis in athletics, and attention was called to the action of the psoas muscles as a possible factor in producing trauma to the appendix. The paper was, "On the Traumatic Origin of Appendicitis."

Dr. J. Henry Barbat: There seems to be a lack of uniformity of opinion, which I think is due to the fact that these cases are not properly analyzed. We find appendicitis in two classes of individuals, one embracing the majority of cases in strong, robust individuals, the other in weakly children, usually of tubercular tendencies. In the appendices removed from weakly children, we rarely find concretions, but instead find that the adenoid tissue is diseased in the same manner as we find in these same individuals in the nose and throat. In many of these cases we can find tubercle bacilli both in the appendix and in the mesenteric glands. Whether it starts in the appendix or in the glands I am not prepared to say. In one case on which I operated, the daughter of a physician, the appendix became suddenly affected two weeks after a very intense infection of the cervical glands. The soreness and swelling left the glands the day following the operation. It appeared like a metastasis. Now in strong individuals we do not find the disease of this character. The strong person eats all manner of food, which is apt to scratch the delicate mucus membrane of the bowel and produce capillary hemorrhages. Particles of rough, undigested food entering the appendix will produce the same condition, and predispose to infection. If anything obstructs the free drainage of the appendix, the toxins are retained, and infection results.

Dr. Winterberg: I wish to say a few words regarding this new theory without bringing in anything in the shape of an argument. I have been astonished that there has not been any one who has made a classification of the different forms of appendicitis. It is a well-known fact that all cases are not alike. Mild forms of catarrhal appendicitis are probably the result of improper feeding, just as we get catarrhal inflammation of other portions of the alimentary canal. They get well as soon as they are put on proper diet. Then we see cases in which adhesions complicate the catarrh, or chronic suppuration sets in, and finally there are cases in which the principal symptom from the start is septicemia, and which rapidly terminates fatally. It is certainly a fact that there is more than one cause for the different varieties. The question is, What is the cause? Appendicitis may be produced just as well by the presence of foreign body, or by hardened fecal matter, as by the interference with the circulation, or by capillary hemorrhage. Dr. Stapler did not go into details about the capillary hemorrhage; he did not say anything about its modus operandi. Simply said capillary hemorrhage, which can not convince the thinking mind. When we operate we find various conditions, and it was rather a revelation to me to hear that in fatal cases we never find foreign bodies. I remember a fatal case in which I found a foreign body which undoubtedly had been the cause of the disease. Because we do not find such things in every case, that does not prove that these things do not at times cause appendicitis. I can not at all agree with Dr. Stapler in claiming that capillary hemorrhage is the cause of appendicitis which explains every case.

Dr. Dudley Tait: Dr. Stapler has only offered a personal opinion, without substantiating it with any pathologic or experimental proof. Riedel, of Berlin, first suggested the capillary hemorrhage theory in appendicitis. The rough handling of the normal appendix, its ligation especially, would, most invariably, cause numerous small venous hemorrhages in the submucosa. The frequency of appendectomy in female nurses depends more upon the composition of the surgical staff of a hospital than upon any other factor. Dr. Stapler's theory is not confirmed by the necropsy findings in cases of submersion, strangulation, etc.

Dr. Stapler: First of all, I want to thank the members who have taken such an interest in my paper. I thank Dr. Huntington, who brought up these late experiments. If Fraser could produce these results by simple ligation, it is only due to the fact that through the ligation the capillary hemorrhage occurred, and after infection ensued. The severity of these cases depends entirely upon the nature of this infection. The colon bacillus can be a very severe one. I am very glad that he brought up the fact that foreign bodies do not cause appendicitis unless infection ensues. As I said in regard to Dr. Rosenstirn, I would say that the foreign bodies do not always cause capillary hemorrhage. It depends upon the shape of the foreign body. When necrosis from pressure occurs, then infection is likely to ensue, and appendicitis will follow. He asks about microscopic demonstrations. I said in the beginning that I have not many cases, but after I presented this same paper at the German Medical Society fourteen months ago, I received the No. 31 "Berliner Medicin. Wochenschrift." Here Riedel (Tena) stated that he had examined 313 cases of appendicitis, and he found in all these cases granulations. The only difference in the conclusions between Riedel and myself are that Riedel says the granulations exist first, and afterward the hemorrhage occurs, whereas I believe that the first thing is the capillary hemorrhages. After this the little loss of tissue ensues, which is, owing to the condition of the processus vermiformus, transformed into small granulations. There is no infection without any primary lesion in pathology. Capillary hemorrhages occur in most diseases. Dr. Tait states that we find that capillary hemorrhage ensues after rough handling of the appendix, but he denies a few minutes later that it is possible to have hemorrhage from overexertion. It is not necessary that every capillary hemorrhage becomes infected. It depends upon the general condition of the patient and the conditions in the processus vermiformis. He says also that tuberculosis can pass the intestines into the abdominal cavity, even in a healthy state. It is very hard to believe this. To Dr. Hunkin's remarks I would say that overexertion is only a relative factor. For a weak man, slight work is already overexertion, when the strong one would not feel even the least fatigue. You all know that athletes are not born, but they train themselves gradually. The large number who are not well trained are frequently affected with appendicitis. If you compare the number of working classes of men and women, you will find that the percentage is the same in men as in women. Dr. Power says he thinks, if constipation is an important factor, then we should expect in constipation more cases. In fact, constipation is predisposing for appendicitis, but as constipation is a chronic disease, it seems that the organism adopts himself gradually to this state. If the strain is too great, however, then a capillary hemorrhage may occur and an infection take place.

CALIFORNIA ACADEMY OF MEDICINE

Regular Meeting, August, 1903.

The president, Dr. D. W. Montgomery, in the chair.

Dr. T. W. Huntington gave an

ABSTRACT OF A PRELIMINARY REPORT OF A CASE OF BONE FILLING.

The patient was a male, aged twenty, who for two months had suffered from a subacute osteomyelitis of carpal end of right radius. Symptoms present were pain, swelling, tenderness, and almost complete suspension of function of wrist, hand, and fingers. The bone was opened over the area shown by the X-ray to be affected. A cavity containing about two drachms of pus was emptied and carefully sterilized. The cavity was then packed with granular calcined bone powder and the wound closed. A small amount of the filling extruded and remains buried beneath the soft tissues. This caused pressure necessitating removal of the skin sutures. The patient was exhibited on the sixteenth day after operation, showing a well healed wound. Pain and tenderness had disappeared and joint and hand function fully restored. Reference was made to the literature bearing upon this method of treatment of bony defects, to the work done by Dr. W. I. Terry in a similar direction, also to the published cases of Dr. Julius Rosenstirn, in which plaster of Paris was employed for the purpose of supplying bone defects.

Dr. H. M. Sherman: What I have to say must be based upon no experience, and is rather a series of questions than an opinion. I do not understand what Dr. Huntington means by sterile pus from a bone abscess. Whenever I get hold of pus out of a bone abscess and it is sterile from pyogens, I conclude that there must have been some tuberculosis in there. We may see such cases where the abscess passes into a quiet stage. As far as the cavity is concerned, if we could be absolutely certain that we had entirely gone outside all of the infected tissue so that there was no off-shoot, I should think that the filling of this cavity was an ideal method, but I have tried so many times to have a cavity absolutely clean and have taken out the whole wall, leaving nothing, and swabbing it with carbolic acid and alcohol, and then have found that there was still another focus, so that I am in doubt about filling with solid materials like this. As it is not uncommon to have these things show themselves, I always want to keep the cavity in view rather than to fill it up. I think that these cavities can be kept free from the pyogens, and can be made to heal in time with granulations. I tried on some dogs to fill these cavities with glycerin agar, and found that it was absorbed and the space taken by granulation tissue, and it acted very well. My only fear is that any substance which I can put into a cavity may cover a focus and so enclose pathologic tissue.

Dr. W. I. Terry: Some time ago I saw Henley. of Breslau, use a suspension of bone ash finely pulverized and injected for ununited fractures. It seems to me that this is a field which is open to the use of bone ash. I tried it in one case, but without success. I injected with a large syringe beneath the periosteum, with the hopes of provoking periosteal growth. It seems to me that the use of the calcined bone as prepared for Dr. Huntington is even better than the use of the finely pulverized ash. Larger particles can be introduced beneath the periosteum, or at the site of the ununited fracture and may produce sufficient growth to bring about a healing. I should not be afraid to use this in these cases, nor would I consider it a foreign body.

Dr. Huntington: With regard to the questions that were raised by Dr. Sherman I am prepared to state that the original infection, while not demonstrable, was, considering the pulmonary attack preceding the bone affection, probably due to the presence of one of the pyogenic cocci. It is well understood that pus retained in a cavity for a sufficient length of time loses it infectivity and becomes sterile. I am strongly of the opinion that this was the condition in the present case. With regard to the second point made by Dr. Sherman: It is true that bone cavities do heal with tolerable certainty by the slow process of granulation, but my experience is that such cavities, after a period of weeks or months, become the seat of at least mild infection, if not worse. This seems almost inevitable. I can not believe that the procedure advocated by Dr. Hunkin is altogether justifiable, particularly in those cases in which the lesion is contiguous to joint structures. It would have been impossible in this case to have removed a large amount of the bony structure surrounding this cavity without materially interfering with the function of the limb. Referring to the general subject of repairing bony deficiencies by the introduction of artificial matter it is to be said that the results have not been altogether satisfactory. There can be no doubt that the one obstacle more than any other which stands in the way of complete success along this line is the presence of infection. If, however, it be possible to sterilize a bone cavity completely and then fill it with the material here suggested, thereby securing a rapid result, a long step in advance of what has been previously done will be made. Again, it is not altogether impossible that tubercular foci in their incipiency may be located and cleaned out and the resulting cavity repaired in a similar manner.

A DOUBLE AMPUTATION OF LEGS, DONE TWELVE YEARS, AND INCIDENTALLY A NEW ARTIFICIAL LEG.

Dr. Huntington presented a case of double amputation of the legs for the special purpose of calling attention to the artificial limbs which have been found available. Dr. Huntington stated that this patient underwent double amputation of the legs sixteen years ago. Wounds were rather slow in healing, but he now has two good stumps, and is walking on artificial legs of his own make. After a long and unsatisfactory experience with various artificial limb-makers, he succeeded in devising those which he now wears with extreme comfort. Chief points of merit in these legs are that the sockets are made of rawhide. He first makes a cast of the stump, including the tuberosities of the tibia and fibula. The socket is then molded upon the cast and when the limb is completed, it is found that the bearing of the stump is chiefly upon the tuberosities. In a long experience in observation of artificial limb results, I have never seen so perfect locomotion in a double amputation as in this case.

Dr. D. W. Montgomery read a report of a case of

EPITHELIOMA OF THE PENIS.

[Published at page 383.]

Dr. Montgomery: In regard to the operations on the cancer in this case, they were successful in removing what they intended to remove. The infection of the lymphatic glands took place before the operation on the penis, so that they could not have been influenced at all by this operation. The infective material had gone out of the sphere of these operations entirely. The argument of the paper is that the operations done, did all they were intended to accomplish, and probably did everything that could have been accomplished.

COOPER COLLEGE SCIENCE CLUB.

Sept. 7, 1903.

Dr. Hirschfelder presented a case of

EPITHELIOMA OF THE NOSE CURED BY THE X-RAY.

Dr. Hirschfelder said, speaking of X-ray in connection with other forms of carcinoma: Dr. Chismore operated on a case of epithelioma of the prostate, and the cancer spread. I was asked to see the case, and it was clearly a case of extention of carcinoma into the surrounding tissues, as had been recognized by Dr. Chismore. Nothing could be done surgically, and the X-ray was suggested. We had a tube made with a long projection that had been suggested by Dr. Margaret Cleves, of New York. The only possible result it may have had was that the patient was practically without pain, although carcinoma in that region is generally very painful. Upon the growth of the carcinoma there was no effect. There was no postmortem on this case. The cases of superficial epithelioma are frequently cured, and the results are sometimes quite remarkable.

CASES OF HEART DISEASE.

Dr. Harris showed several heart cases. He said: "All of these cases bring up some particular feature concerning the heart, from which view-point they will be considered. The first case is of simple hypertrophy in exophthalmic goitre. The patient has suffered for three and a half years. Began with marked tachycardia and severe neuralgic pains. When I first saw him, seventeen months ago, the most distressing feature was this pain. At that time the pulse rate was about twenty-one to the quarter minute. An examination of the urine showed very marked traces of sugar. Under treatment, consisting of a lessened carbohydrate diet, laudanum, and application of electrodes to the cervical sympathetics, he improved, gaining sixteen pounds in six weeks. While improvement was still continuing, he allowed himself to become much excited concerning change handed him by a street-car conductor, and following this a relapse occurred. In spite of all treatment, he lost four pounds in seven days and showed marked tachycardia. The patient was put in Lane Hospital and kept there from June 3 to 30, 1902. During these four weeks the heart rate varied between ninety-six and one hundred and thirty. During the past fifteen months the patient has been much improved, and there has been no tachycardia. When the patient was first seen, the area of relative cardiac dulness extended 12½ c. m. to the left and 4 c. m. to the right of the mid-sternal line. At present he shows the same area of dulness. The point to which attention is directed is that in spite of an absence of overactivity of the heart muscle, for over fifteen months there has been no decrease in size of that organ. Dilatation has never been present. We are accustomed, in thinking of a skeletal muscle, to associate a cessation of overactivity, granting that nutrition be good. with a decrease in size. This does not seem to be the case in the hypertrophied heart of exophthalmic goitre. The text-books say nothing concerning the decrease in the size of the heart in this disease. The next case is one which we have seen for two years. It is a case of heart disease following acute articular rheumatism. The patient's first attack of this latter disease occurred ten years ago. It presented no special features. At that time he noticed no cardiac involvement. Since then he has had numerous attacks of acute articular rheumatism. He now presents himself with marked signs of aortic insufficiency, but with special features. His urine shows the presence of a chronic nephritis, and the stomach contents, after giving a test meal, show the presence of a chronic gastritis. The arteries show marked grade of arterio-sclerosis. The special feature is the severe pain located about the pericardium and extending into the pit of the stomach, up into the neck, and sometimes into the left arm. Of late such attacks have been growing more frequent, occurring several times during the day and night, lasting from one-half to two hours. With the pain there is much belching of gas and palpitation. The physical examination of the patient's chest shows a marked hypertrophy of the heart. Inspection fails to reveal any pulsation attributable to an aneurysm. By percussion, the shape and size of the heart are found to be suggestive of aortic insufficiency, but by percussing over the right upper chest near the sternum, we find a well-marked area of relative dulness in the second and third intercostal spaces. There is an absence of a thrill over the apex or base. On auscultation at the apex, one hears the first sound accompanied by a soft systolic murmur, followed by a long, blowing, sometimes blubbering, diastatic murmur. Proceeding upwards and inwards, this to and fro character of heart sounds is maintained, being particularly well heard over the aortic area, and heard also over the area of dulness in the first and second right intercostal spaces. The radial pulses are synchronous and typically Corrigan in quality. There can therefore be no doubt of the existence of an aortic insufficiency. The systolic murmur over the aortic area is attributed to a sclerotic condition rather than to a definite aortic stenosis. It remains for us yet to determine the nature of the severe pain from which the patient suffers. It should further be said that these pains come c especially after eating, at arising in the morning, and at the slighest mental excitement. This history, together with our findings of a very sclerotic artery and of an aortic insufficiency, justify the diagnosis of angina pectoris. There are not sufficient physical signs or symptoms on which to base the diagnosis of aortic aneurysm. The dulness found in the upper right chest could well be explained as being due to a dilated sclerotic arch of the aorta. The diagnosis in the complicated case would then be aortic insufficiency, dilated arch of the aorta, arterio-sclerosis, chronic nephritis, chronic gastritis, and angina pectoris. A few words concerning his treatment. The spirits of nitroglycerin have been used, and at first afforded marked relief. The patient has increased his daily dose of this drug till he now takes as much 320 drops in a day. The largest dose that I am able to find in the literature is recorded by Sajous, being ʒT of a ten per cent solution. In San Francisco, where so many cases of illuminating gas poisoning occur, it is not inappropriate to recall the beneficial action of this drug hypodermically in such cases. A few months ago the patient was put in Lane Hospital, where a new procedure was tried, one first suggested by Askanazy, in 1895. Diuretin in ten-grain doses was given four times a day. This acted very well, lessening the pain until a fresh attack of acute articular rheumatism interfered with its benefits. After his recovery from this attack, the patient left th hospital, and since that time this treatment has given only an indifferent result. The third case is particularly interesting from the extent of involvement. The patient has had only two attacks of acute articular rheumatism, the first one ten years ago, the latter one three months ago. Without going into further detail, we proceed to the essential features of the examination. The pulse is readily seen in the radicals, is Corrigan in quality, but of small volume; being a small pulse, the artery wall is readily felt. Inspection shows marked pulsation in the vessels of the neck, and very marked cardiac impulse. The point of maximum impulse is situated in the sixth left intercostal space, 16 cm. from the mid-sternal line, and the relative cardiac dulness begins above at the third rib, extends 16½ cm. to the left, and 5 cm. to the right of the mid-sternal line. A definite thrill is felt over the aortic area, formerly more marked than at present. On auscultation at the apex, one hears the first sound followed by a squeaky, definitely musical murmur. Diastole is occupied by a long blubbering murmur. The systolic murmur just referred to is transmitted into axilla, and is faintly heard in the back. Proceeding inwards toward the sternum at the fifth interspace, the diastolic murmur is well heard, while a systolic murmur is heard of entirely different quality than the one heard at th apex, being soft and blowing. The to and fro character of heart sounds is heard well along the sternum and over the base. At the aortic area, the latter described, soft, systolic murmur is of maximum intensity and is transmitted into the vessels of the neck, and into the right subclavian artery. The pulse, the thrill over the aortic area, the systolic murmur, different in quality from that heard at the apex, justified the diagnosis of a slight grade of aortic stenosis. Aortic insufficiency is a marked feature of the case, and an insufficiency of the mitral valves is also certainly present. As to the existence of a mitral stenosis, we are not convinced. The patient also shows by the examination of his urine a chronic nephritis. That he has gotten along so well with his combined valvular lesions and his very large heart is in part explained by the fact that he is a barber, an occupation not requiring excessive muscular effort. It should be added that two years ago the patient contracted a genital chancre, for which he was treated at the time. A marked periosteal node developed on his left humerus three months ago, which disappeared entirely under antiluetic treatment. The time allotted explains the short consideration of this most interesting case.

Dr. J. O. Hirschfelder: The interesting point with the second case is the attacks of angina. It is a mistake to presume that angina pectoris is always caused by sclerosis of the coronary arteries. In a large number of cases angina pectoris is a struggle of the heart against pressure. So you have this pain in the heart and radiating from the heart in cases where there is a resistance to be overcome that the heart can not meet. With regard to large doses of nitroglycerin, at the City and County Hospital we had gone beyond the top record of this man's dose. We have given seven and one-half grains of nitroglycerin a day, and then we stopped because we were disturbed about giving more.

DISEASES OF HEART; EXHIBITION OF SPECIMENS.

Dr. Wm. Ophuls showed some specimens of hearts from the museum, among them were a very much hypertrophied heart in a case of granular atrophy of the kidneys, and another one with marked, although not quite so pronounced, hypertrophy in chronic parenchymatous nephritis. Dr. Ophuls said that the heart muscle in cases of cardiac hypertrophy, outside of a slight amount of fatty degeneration or atrophy, frequently showed nothing microscopically to account for the marked cardiac manifestations clinically. Dr. Ophuls exhibited also about twenty specimens of chronic septic or verrucous endocarditis. They were all produced by infection with the common pyogenic cocci, mostly pneumococci, streptococci. In a number of these cases shown there were large patches in the lesions that were calcified, which at times made them resemble ordinary athermatous lesions of the valves very closely. Some of the specimens were interesting by exhibiting a marked tendency to repair, even after extensive destruction of the valves. There were two specimens of chronic septic endocarditis aortica in which two flaps had been partly destroyed and then grown together to form one larger valve in place of the two original ones, the only evidence of the former disease being a thin ridge where the two flaps came together. In connection with these cases were shown several examples of more acute lesions, one with valvular aneurism. Among the more acute cases was one case which was the type of gonorrheal endocarditis, the germ in this case being the staphylococcus, and not the gonococcus, but the resulting lesion was very much like those in gonorrheal endocarditis. Regarding endocarditis in acute articular rheumatism, Dr. Ophuls said he had never been able to discover a micro-organism in the few cases examined by him, the difficulties were that so few of them came to post-mortem early in the disease. He said he had had one typical case recently, and cover slips made from the valves showed no germ on microscopic study. He said he was positive that what was commonly called acute articular rheumatism was not an etiological entity, many cases of chronic sepsis f. i. being mixed up with it. There was also a case of extreme atheromatous stenosis of the aortic valves and a typical button-hole stenosis of the mitral. Some of these cases were accompanied by clinical histories and sphygmographic tracings which were demonstrated, and the results and the clinical examinations compared with the findings after death.

SAN FRANCISCO SOCIETY OF EYE EAR NOSE AND THROAT SURGEONS.

May, 1903.

The president, Dr. Louis C. Deane, in the chair.

Dr. R. W. Payne read a paper on

THE ADVANTAGES OF MULE'S OPERATION OVER SIMPLE ENUCLEATION.

The President: This has been a most instructive and interesting paper. The only objection I have to offer is that the doctor has used the term simple technique to cover a large number of interesting facts. I should be pleased to have him go more fully into the subject.

Dr. R. W. Payne: What I mean by simple technique is that the operation may be done with the least possible traumatism. To accomplish this one desirable step in the operation is to keep the eyeball in position by using a long steel pin to transfix the sclera just behind the limbus, one pin above and one below. I find the cataract blade best for beginning the amputation of the cornea and a small scissors to complete it, the incision made at the limbus. The sclera is emptied with as little violence as possible. By introducing a probe upon the end of which a cotton ball is rolled loosely, one engages all the inter-ocular structures by twisting it around. This torsion empties the sclera completely, unless there has been extensive inter-ocular inflammation, in which case some exudate may be adherent to the sclera, requiring gentle curettage. Hot bichlorid solution, 1 to 6,000, is sufficient to arrest hemorrhage and sterilize the field. The glass ball is cleansed with alcohol and left standing in 1 to 2,000 bichlorid before use. Sometimes the cornea is small, and its amputation leaves an opening not large enough to introduce a ball. Here a nick with the scissors on the side admits the ball readily. Careful approximation of the cut edges of the sclera and conjunctiva with silk is all I find necessary. The sclera heals slowly and requires silk. I allow the sutures to remain ten days, and usually find most of them cut out by that time. I think it is a mistake to imbed catgut sutures to bring the scleral wound together, and then the conjunctiva over them. They are sometimes not completely absorbed, and leave an irritable point in the stump. There should be no tension across the glass ball, and still the sclera should be sufficiently filled to keep its form. An important point is to prepare for the reaction which follows these operations, which is very great if much violence is used. The lids are carefully replaced over the stump, and the orbital opening carefully filled in all around with a cushion of absorbent cotton, which is held in place by a snug bandage. In the first twenty-four hours there will probably be some pain. It is usually relieved with one hypo. The bandage is not removed until the fourth or fifth day, if things go well. The patient can leave the hospital at the end of a week, and at the end of two weeks the case is in a very good condition, though not ready yet for the shell.

Dr. K. Pischel: I have performed Mule's operation only a few times with good success. But I must confess I am always somewhat afraid of sympathetic inflammation. Regarding the appearance, the hollow artificial eyes, if well made, they fulfil all reasonable demands. I would like to have it explained why the orbit of a child should develop when a glass ball is worn, and vice versa.

Dr. F. B. Eaton mentioned the use of paraffin injected into the capsule of tenon as a substitute for evisceration of the globe.

Dr. A. G. Wiborn: I have seen eighteen or twenty cases. There were three failures, one excessive reaction from carbolic acid caused the ball to come out, another the globe was a little too large and broke the stitches and came out. In the third, the stitches did not hold and the wound gaped the least bit, and so for about eight months caused the globe to look very much like a dilated pupil. The other cases were very satisfactory, giving unusually free movement and nice appearance. Of course, favorable cases were selected for the tests. Reaction is very violent when carbolic is used, even when alcohol follows, and ice is necessary for forty-eight hours or so.

Dr. C. S. B. Nagel: If I remember, we were discussing this subject at the last meeting. My experience has been with a modification of Mule's operation. I use a silver wire ball instead of the glass ball, and I've never had one come out. One of the points in the operation is not to get too large a ball. I have a very favorable opinion of the operation.

Dr. L. C. Deane: Much has been said against the Mule's operation, but to me it should rank as one of the classic operations when performed in selected cases. At this meeting thirty-five cases have been reported by several of our members, and out of that number only four cases were considered failures. The satisfactory results that some of our members have had is most eloquent in its plea for the operation, more so than the scattered reports we gather from various countries and journals.

Dr. Payne: The members will note that in the cases I selected for the operation, there was practically no danger of a sympathetic ophthalmitis. All these cases reported were successful, and not one has had any trouble with the stump. So far as I can gather, Mule's operation has not been followed any more often by sympathetic trouble than has enucleation, and I think it is our duty to make an effort to get the best possible cosmetic effect for the patient without endangering the other eye. This we can do with the Mule's operation by using care in selecting our cases.

Dr. Eaton remarked that the reports on the Mule's operation from various parts of the globe have been antagonistic and the general mass of evidence against it, etc.

Dr. Payne: Dr. Eaton has quoted what the vast experience of oculists has been. I admit that the reports of isolated cases have been unsatisfactory, many oculists abandoning and condemning the operation after an experience of two or three cases. Dr. Wiborn reports twenty cases with three failures. Two of these were improperly selected for the operation. The point I wish to make is that with care in selecting cases, and with greater care in technique, our results will be as good as in simple enucleation, and a better cosmetic effect obtained. Mule's operation is for selected cases, and has certain contra-indications. The cases mentioned by Dr. Frederick plainly indicate enucleation and should not have been selected for the Mule's operation.

BOOK REVIEWS.

"The American Journal of Orthopedic Surgery." The first (the August) number of the first volume of this publication came to our table recently. It is the intention of the American Orthopedic Association to publish the journal quarterly, replacing with it the Transactions. The personnel of this editorial committee, consisting of R. W. Lovett, M. D. (chairman), Boston, Mass.; B. E. McKenzie, M. D., Toronto, Canada; H. M. Sherman, M. D., San Francisco, Cal., bespeaks the excellence of the journal. This first issue consists of over one hundred pages, original articles, illustrations, and abstracts. If the succeeding numbers are of similar quality, which is expected, this journal should have an extensive subscription list among the specialists in orthopedics, as well as the general surgeon. The subscription is five dollars per year in advance.

"Diseases of the Ear." A text-book for practitioners and students of medicine, by Edward Bradford Dench, Ph. B., M. D. Professor of diseases of the ear in the University and Bellevue Hospital Medical College; Aural Surgeon, New York Eye and Ear Infirmary; Consulting Otologist to St. Luke's Hospital; Consulting Otologist to the New York Orthopedic Dispensary and Hospital; Fellow of the American Otological Society, of the New York Academy of Medicine, of the New York Otological Society, of the New York County Medical Society, etc. With fifteen plates and one hundred and fifty-eight illustrations in the text. Third edition. Revised and enlarged. Price in cloth, $5.00.

D. Appleton & Co.
New York and London.

Revisions of standard works, when thoroughly and completely done, are in order. Especially will this apply to the excellent volume here mentioned. The third revised edition, with fifteen plates, and one hundred and fifty-eight illustrations in the text, enlarged and almost entirely rewritten will meet the requirements of the general practitioner as well as those of the general surgeon. Surgery in every branch has made phenomenal strides no less in otology than any other branch, and operative treatment of chronic suppurations of the middle ear has reached such a stage in its technique that any work of ten, or even less number of years ago can not be authoritative to-day. The same holds good with reference to the itracranial complications following such chronic complications. The chapters devoted to these disease conditions have received extended treatment at the hands of the author and will well repay careful reading and study. Two colored plates were made expressly for this work, after dissections by the author, to show the anatomy of the internal jugular vein and the technique of exploratory craniotomy. The index is elaborate and correct; binding and paper excellent.